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ORIGINAL ARTICLES.

THE MANAGEMENT OF EXTRA-UTERINE PREGNANCY.

Read in the Section on Obstetrics and Gynecology at the Thirty-ninth Annual Meeting of the American Medical Association, Cincinnati, May, 1888.

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The history of the management of extra-uterine pregnancy, like every other conception which has finally resulted in good to the human race, begins with very crude—I might almost say utterly empirical manœuvres. Slowly it has emerged from its horribly uninviting chrysalis to its crawling caterpillar stage, which is almost passed, and its bright-winged life has now really begun.

Passing over without mention the mistakes and blunders of those who used to evacuate the amniotic fluid, and those who once injected substances into the gestation sac, as well as that later and better procedure, elytrotomy, let us go at once to the discussion of the only two methods which now receive much recognition from the medical world, which are electricity and abdominal section.

Let me state clearly and distinctly in regard to the electric current in all its forms, that I believe it wrong in principle, dangerous in practice, and frequently disastrous in its final results.

One of the assumptions of the originators of the electric treatment was that all one need do to stop the development of the gestation sac, was to kill the fœtus, believing that on its life alone depended not only the secretions of the amniotic sac, but the growth and development of the placenta. That they in reality were nothing but what their ordinary names, fœtal appendages, indicate, and that, like the nails and hair to the general body, the death of the embryo meant the immediate dissolution of all. This, however, thanks to Berry Hart, Knowsley Thornton and Tait, we know is not true, for the placenta frequently will live on, and continue developing, long after the premature death of the fœtus.[1]

This agrees most fully with all my studies on the endometrium, the published results of which are contained in my papers on "The Menstrual Organ," "The Endometrium in the Cycle of the Gut," and "The Infantile Uterus." The central idea that runs through them all is, that the placental matrix has an entirely separate and distinct physiological existence from all other organs, and that its function is to take up and nourish the ovum, which is done entirely independently of control from any other source than its own nerve supply.

It is true this nourishment cannot be done until the ovum is present; it is also true that the mill cannot grind without the wheat, but no one would think for an instant that the wheat controlled the movements of the mill. In the intra-uterine condition we have a totally different state of affairs from that of the ectopic form. In the latter we have the placenta developing from the adenoid layer of the tube, but we do not have it surrounded by a powerful muscular sentinel, who is ready, on the slightest indication of anything wrong, to expel the whole of the new development.

How frequently it is our experience to find signs of life in only two or three months' abortions; so much so, that it is an egregious blunder to think every miscarriage is due to some error either in the fœtus or placenta, for experience has long since taught us that repeated abortions are due as much to some conditions in the uterine wall, as they are to diseases of either the fœtus or placenta.

In extra-uterine pregnancy, however, an abortion is impossible. The placenta, once started, must go on until it has finished its physiological life, the limit of which is nine months. It is the gradual death of the placenta that causes the terrible struggles of the extra-uterine fœtus, at the close of the false labor, which, so far as the child is concerned, is nothing more or less than a slow asphyxia.

Thus I am thoroughly convinced that, to stop the growth of the ectopic gestation sac, it is necessary not only to kill the fœtus, but also to shock the pelvic sympathetic so thoroughly as to cause an interference with its control and produce what, in intra-uterine conditions, would result in a menstrual decidua. Thus, gentlemen, you see why it is that the reported cases of electric treatment of extra-uterine pregnancy are so stubborn in

[1] Tait. American Journal of Obstetrics, April, 1888, p. 394. Berry Hart. London Obstetrical Society.

their resistance, and you see the reason for the necessity of so many repetitions of those terrible doses of electricity before there is any appreciable diminution in the tension of the sac. In all probability most children are killed by the first application, but it is the placental membranes which keep up the secretion of the amniotic fluid, and it is them you must kill before any diminution can be looked for.

Thus it seems to me to be fully demonstrated that it is not the fœtal, but the maternal nervous system which must be overpowered by the battery, and for this reason I believe that the whole practice is founded on a dangerous error. Another of their assumptions, from personal experience, I can assert most positively is wholly wrong, and that is as to what the so-called "premonitory symptoms" mean.

On page 293 of the *Transactions of the American Gynæcological Society*, for 1887, you will find a discussion of this subject, introduced by our honored chairman, in which all but Dr. J. E. Janvrin concurred in the belief that the "colicky pains," and "first slight shocks," are due to contractions of the tube, or movements of the fœtus. He, however, took the ground that they are due to the giving way of some structure, and that in most cases it is accompanied with hæmorrhage to a greater or less degree. His speech closed the half hour allotted for the debate of papers, or I would be on record beside him. But what our President then cut me off from I here wish to say at length, and that is, that Dr. Janvrin's ideas are correct.

I have now, either as assistant or principal, officiated on about six ruptured extra-uterine pregnancies, and in every one of them we found clots of different ages. My own case, published in the *Medical Record* of February 26, 1887, showed this to a very striking degree, but to make sure that I was correct about the cases in which I assisted Mr. Tait, I wrote to him for information on that point, and here is his reply:

"7 THE CRESCENT, BIRMINGHAM, APRIL 12, 1888.

"*My dear Johnstone*:—To my horror I have just found your letter of February 26, under a mass of papers in my desk, unanswered. It is, I find, the most fascinating occupation that gynecologists can indulge, and just in proportion to their want of experience they dogmatize on such points as this you ask information about. As a rule, there are no symptoms of tubal pregnancy sufficient to call for medical interference, or even examination, until the time of rupture. The proof of this is that, now having seen seventy cases, I have only once had occasion to make an examination before the period of rupture, and then the symptoms were those of ordinary tubal occlusion, nothing to indicate pregnancy. The next time I saw the patient the tube had ruptured. The rupture I diagnosed, but I thought it was a pyosalpinx which had burst, and caused general peritonitis. I operated immediately and found a large blood clot in the abdomen. I removed the ruptured tubal pregnancy and the patient got well.

"Now if there is anybody else with an experience as large as this who can give evidence to the contrary, we may begin to listen to him, but until such evidence appears, I think it may be reasonably supposed that the opportunities for diagnosis of tubal pregnancy, before rupture, are very rare, and the difficulties of recognition immense. In my preparations the point which you speak of is abundantly proved. There are successive hæmorrhages which leave their relative dates recorded in the altered blood clots. This you saw in your own experience with me here. I don't know how you are getting on with this subject on your side, but here we are going through the stages of the old story. At first the senile authorities stated, of course, that our cases never occurred, and now they are contradicting those of us who have had experience upon the most primitive facts of the record, utterly unable to unlearn their old notions, though following us in the new. It is very likely, of course, to be the same with you.

"I am much interested in what you say about the rush for electrolysis in your country, because here it is already exploded, and will rapidly subside into its original position of rampant quackery. You are at perfect liberty to make any use you like of this letter. Yours ever,

"LAWSON TAIT."

This, I think, places it beyond all doubt that Dr. Janvrin is not alone in the experience which he reports. But to me it is the rule, and not the exception, that when we are first called to cases a slight rupture has already begun. But, as in the one out of seventy which Mr. Tait saw before rupture, it is almost impossible to tell whether the tube contains pus, serum, or a fœtus. So, in the use of electricity, we are not only almost sure to begin its use in cases where rupture has already begun, but we can never be absolutely sure that we are not passing it through some inflammatory mass, like that case in which one of my critics[2] deluded himself into thinking that he had absorbed the whole of a five months' gestation sac, and all in the space of three weeks. Several less lucky cases were brought out in the debate on this subject before the British Medical Association, at its meeting in 1886. Electricity was used on inflammatory masses, and instead of absorption, suppuration took place, and prompt laparotomies were all that saved the patients.

Having, as I think, pretty fully shown the errors on which its practice is based, let us turn to the immediate dangers of its use.

Fortunately, the aggregate of the cases on

[2] The American Journal of Obstetrics, August, 1887, p. 818.

which it has been tried is small, or before this we would have heard of a case of syncope, but to get it, all we have to do is to continue passing these powerful currents through the pelvic sympathetic, and sooner or later we will be sure to find a weakened heart that will not stand it. How many unreported cases like that of Dr. Janvrin the world has contained no one can tell. How many suppurations of effused blood, inflammatory masses, peritonitis, and the like, have been justly chargeable to the electric current, is a question that will probably never be solved, but from Dr. Chadwick's and others' experience with Apostoli's method of managing uterine myomata, to say the least of it, it is a condition in which we would not be at all surprised at having inflammation as a result.

An extra-uterine pregnancy never occurs in a normal tube. Whether you accept the old-time doctrine, that it is always due to a tubal occlusion, or whether you take the theory that it is frequently caused by desquamative salpingitis, which strips the tube of its celliated epithelium, laying bare its subjacent adenoid layer, which not only robs the ovum of one of its means of transportation, but exposes to its stimulation the natural placental matrix; in either case, we have the gestation sac contiguous to some old inflammatory centres.

We have long since known that weak currents have a considerable absorbtive power, but we have also known that strong ones are extremely irritating. So it seems to me that he who passes the full force of a 20-cell battery through a fresh effusion of blood, among a lot of old adhesions and cicatrices, with a possibility of a slight leakage of amniotic fluid, need not be in the least surprised to find a violent inflammation following his manipulations.

But granting that he does find his case before any blood has been effused (which remember, in Mr. Tait's experience has occurred but once in seventy cases), to shake up the smouldering embers of an old conflagration by what is almost a caustic application seems to me, to say the least of it, an extremely daring procedure. It is true that some cases have stood it, but it is also true that pregnant women have been known to fall from the third story, and repair double fracture of both femurs, without aborting, but still we do not consider it exactly the thing to pitch them out of these windows, just to see if they can all stand it.

A case has already been recorded in which a slight rupture was the means of the diagnosis of the case, and in which, in spite of several applications of the electric battery by the best electrician in New York City, the case went on to final rupture and death. I refer to the case of Dr. Janvrin, reported in the *Transactions of the American Gynæcological Society* for 1886, and what I believe is that, unless we stop the use of the electric battery, it is only a question of time until others will be placed beside it. Thus my reasons for believing it is immediately dangerous in practice are, that weakened hearts will not stand the currents we are forced to use, that we rarely ever find a case until a greater or less degree of rupture has taken place, and that the action of these terrible currents on the sac invites the completion of the tears, and that the risk of exciting immediate and violent inflammations and suppurations by thus roughly handling tissues which are already predisposed to them, is, to say the least of it, a very strong possibility.

But the greatest objection of all to the use of the electric current is its remote results. Before we knew anything about tubal diseases, or before electricity was even heard of, it was an established fact that a woman with a dead fœtus in her belly was never out of danger; and now, after all the light that has been thrown on these troubles by the exhaustive study of the various pelvic inflammations, leaving out of sight all the invalidism which they cause, we know that, no matter how many years the mother may survive the fœtus, she is never entirely free from the risk of a septic infection until she is laid away to her final rest.

Cases have been reported of the suppurations of lithopædia after years of apparent quiesence, it is true that such suppurations are very rare, but it is also true that lithopædiæ themselves are now a days seldom formed; many of what used to be called lithopædia we now know to be dermoid production of one sort and another, and by the more advanced methods of managing the pelvis, the errors that lead up to them in Europe are now found and corrected before they are produced, but the mere fact of one of the ultimate aims of electrolysis, being in itself a constant menace to life, seems to me goes a long way to undermine the whole practice.

Long, however, before the fœtus can possibly be calcified is the greatest period of danger. Statistics, like tombstones, can be made to serve almost any purpose, and for that reason I have so far spared you, and do not now propose to inflict upon you any long tables with biased conclusions cut to fit my own ideas, but there is food for thought on this subject which I will give you in a most condensed form from a paper read before the Chicago Gynæcological Society last June, by Dr. Bayard Holmes. It is his statistics, which I especially want. He is authority for saying that "Kiwisch collected a hundred cases of extra-uterine pregnancy of all kinds, and it appears from the summary of these cases given by Bandl that about 37 per cent. of these fœtuses became infected, 17 died of peritonitis, more or less acute, four died of peritonitis after the fœtus had long been retained, nine died through long continued suppuration and perforation, seven recovered after spontaneous elimination. Archer collected 132 cases out of which

about 47 per cent. became infected; Parry's cases show about 28 per cent. of those which had passed full term suppurated. During the first year 12 per cent. of the whole number terminated in suppuration, during the second year 5 per cent., during the third year 2½ per cent. After this time less than 2 per cent. were infected each year." This proves conclusively, to me at least, that while the great majority of suppurations occur shortly after the death of the fœtus, and that though the ratio of deaths diminished rapidly after the so called quiescent state has been reached, still the woman must carry a constant source of danger throughout all the rest of her existence, which may explode at almost any time.

Electricians may say it is not fair to quote such statistics on us, for we never practiced electrical treatment, after the fifth month, and that it is only when the pregnancy is small, before the osseous tissues begin to take the place of the cartillaginous, that we hope to get rapid absorption.

In answer to this, though it can be easily shown that you do get suppuratious, in Dr. Lusk's historic paper before the British Gynæcological Society two years ago, are two deaths of infants at five months, on one of which the electric battery had been used, both of which sloughed out. In the debate referred to on this subject, before the American Gynæcological Society last year, Dr. Chadwick, of Boston, reported another, with like results, in which the electric battery had been used, and what is stranger still, the statistics just referred to by Holmes embrace all kinds of extra-uterine gestation. One more entrenchment the electricians may take, and that is in that old exploded idea, that the safest way of getting rid of a dead fœtus is by suppuration. This Lusk shows to be utter folly. So much for the absolute risks to life, but there is still another (and to me an insuperable objection) and that is the months—and even years of invalidism which this method must entail. Now a days, when we have no hesitancy in removing incurably diseased tubes and ovaries, it seems almost like a crime, to allow a woman to carry a tube, which is not only a constant menace to her life, for months a source of pain and wretchedness, and which for years will keep her from her social duties, to say nothing of the repeated monthly congestions and second pregnancy would be almost the signature of her death warrant. Thus, gentlemen, it seems to me, that the leaving of such a condition in any stage of development, can be fraught with little but mischief to the mother, and all her associates.

I must unhesitatingly say that all extra-uterine gestations, up to the fifth month, ought to be removed through an abdominal incision, as soon as found. The very cases, which the advocates of electricity claim the most for, are the very ones which would be the most easily and safely removed by laparotomy. In proof of this I refer you to Tait's statistics, for I am sure that no other process could possibly have saved as many women, as this last method, which has almost reached perfection at a single bound.

Where a complete rupture has taken place, that is where a vessel large enough to bleed a patient to death, has given way, and which is not held in check by some rapidly adhesive form of peritonitis, not even the most enthusiastic admirers of electricity would for a moment think of using galvanism or of doing anything but following Tait's lead, and when they fully realize that the initial shocks are due to a lighter form of the same condition, I believe they would hesitate before passing their powerful currents through it.

Just on this point let me say that I believe a large proportion of the difference between Tait and his opposers is a misunderstanding in the time when the tear takes place.

Tait knows from his operative experience that the premonitory shocks are nearly always due to a greater or less degree of rupture, and acting on this knowledge, *operates at once*, classing all his cases under the one head of rupture.

The opposite side of the debate, though, assume that these alarming symptoms are due only to contractions of the Fallopian tube, but when they realize, what in this paper I have tried so strenuously to show, that they are in reality dealing with rupture, I believe they will find themselves not so very far after all, from the ground on which Tait has built an everlasting monument. Though leading the van, Tait is not alone in successful European work, on extra-uterine pregnancies, for Berry Hart, Martin and others have reported almost equally good results, and I am glad to say that the profession on this side of the Atlantic are waking to the necessities of the case.

Since the report of my success, cases have been reported by S. C. Gordon, of Portland, Maine; Joseph Price, of Philadelphia, Pa.; William Gardner, Montreal, Canada; G. M. Tuttle, of New York City, and Charles B. Penrose, of Philadelphia, all of which go to show that there are laparotomists in this country that can do the work successfully when they have the opportunity.

But for the exact status of the operation in the different countries, let me refer you to that most invaluable work of Greig Smith on Abdominal Surgery.

Up to the period of the fifth month, I doubt whether elytrotomy ought ever to be done, and after that time I am sure its usefulness will be extremely limited, but we may occasionally find badly neglected suppurating cases, in which we are bound to open them, at the places where they

are about to point, and this will sometimes necessitate an elytrotomy. But as the world grows wiser, I hope these purulent cases will become like vesico-vaginal fistula, and other conditions which are plainly due to neglect, an almost vanishing quantity. The results of operative interference on a viable fœtus, have so far proved disheartening, and as to just what should be done with them, even our most advanced thinkers have not settled to their own satisfaction. It seems to me though, that the longer we let the placenta alone, the wider it will spread out from the centers at which it began, the more numerous its adhesions will become, and the richer will be its blood supply. It is true that a very few of these placentæ reach a higher organization, and when left behind after a laparotomy are gradually absorbed. But their number is so extremely small that I do not believe we ought ever to leave them behind, where it is possible to remove them. My rule, until I see some good reason for changing it, will be in all forms of ectopic gestation, to operate, as soon as I find them, and by these means hope to give the mother the best possible chance.

Martin, of Berlin, has certainly given us, in his one successful "primary" laparotomy, a rule which I think we ought always to attempt to follow, and that is in controlling the blood supply to the placenta, we ought always to apply the ligature to the proximal, and not to the distal part of the vessel.

It is true that in some cases this would be almost impossible, but in many others we would be able to get at the broad ligaments, and thus cut off the blood current before it reaches the soft, treacherous tissue, which lies just underneath the placenta. In closing let me state that after a careful study of the subject, I am convinced that laparotomy is the only thing which ought ever to be thought of as a cure, for extra-uterine pregnancy, and that the time to operate is as soon as possible after its discovery, and if we do not use exploratory incision to clear up our doubtful cases, we are in great danger of letting our patients die without thoroughly understanding the cause of death.

When I began the preparation of this paper I thought I was almost alone in this country in the extreme ground, that a tube containing a small fœtation, ought to be removed as soon as discovered without any further meddling, but I have just seen Dr. Janvrin's paper, of April 16th, before the New York County Medical Association, which shows that he, and quite a number of the younger members of the profession, are taking the same ground that this paper advocated, and speaks of two cases which have already been done by Drs. Howard, Kelly, and Joseph Price, of Philadelphia.

I do not believe the mortality on extra-uterine pregnancies, between the sixth and tenth week—where no rupture has occurred—in the hands of first class operators with the proper surroundings, would be as much as one per cent. The truth is the proper management of extra-uterine pregnancy, is still in its infancy, and is not yet free from the encumbrance with which ovariotomy had so long to struggle. It took years, I might almost say generations, for the ovariotomist to learn not to meddle with his cases, until he was ready to take them out, and they have hardly yet got past the tapping age, but when they have reached those Arcadian days when all ovarian tumors are operated on while the patients are strong and well, by trained men, (who do not rush into it merely as a means to advance them in other branches of their practice) but who have given up to it, not only their time, their money and I might say their very souls, I hope the electric battery will have been relegated to its proper fields, and its use as a fœticide will be remembered merely as one of the transitional stages, in the developing management of extra-uterine pregnancy.

Danville, Kentucky, May 5, 1888.

THE GREAT VALUE OF A 0.25 D. CYLINDER IN THE RELIEF OF HEADACHE AND EYE PAINS.

Read in the Section on Ophthalmology and Otology, at the Thirty-ninth Annual Meeting of the American Medical Association, May, 1888.

BY JULIAN J. CHISOLM, M.D.,

PROFESSOR OF EYE AND EAR DISEASES IN THE UNIVERSITY OF MARYLAND, AND SURGEON-IN-CHIEF OF THE PRESBYTERIAN EYE AND EAR CHARITY HOSPITAL OF BALTIMORE.

In the selection of cylinder glasses, there is a disposition on the part of many to ignore the lesser degrees of astigmatism, as minor eye defects not worthy of recognition. I have heard often the expression from ophthalmic surgeons, that they never prescribe less than 0.5—D. lens, and am led to the belief that a very large number of young patients are allowed to suffer with eye faults uncorrected, because of this opinion. My practice and teaching for many years has been to recognize irregular refractive faults, however small, and by so doing give great comfort. In fact, I find as the result of my own experience, that these lesser faults are often the most complained of, and that the most marked and annoying reflex troubles are occasioned by the *smaller*, and not by the greater degrees of astigmatism. Pain not restricted to the eyes and forehead, but extending down the spine, disturbing the stomach, and even affecting the limbs, anxious troubles which have resisted the most thoughtful care of the physician and the skill of the gynæcologist, have been removed promptly by the use of a 0.25 cylinder lens.

Annoyances from the lower degrees of astigmatism are especially met with among the advanced pupils of the high schools, and more especially in girls. These are often hereditary faults found in the parents as well as in the children: never complained of by the parents who have no cause for eye straining, but brought out conspicuously and annoyingly in the children under forced education. Under the long continued pressure of eye work, which the education of the period demands, very small faults will cause an accumulation of irritation which suddenly explodes, and forces a suspension of study. The continued eye tax of from ten to fourteen hours in the twenty-four will cause the eyes to break down; and then, when the eyes become irritable, even a few minutes of use will bring on eye pains with headache. Eyes heretofore very strong become so sensitive that exposure to daylight is painful, necessitating the use of smoked glasses.

Most of these students have V.=$\frac{20}{20}$, and can read No. 1 of the test types with readiness. Convex glasses would blur distant vision; a very weak concave might sharpen distant outlines. *The painful* eyes of these patients, cured by *weak cylinder* glasses, show that the apparent *perfect vision* V. = $\frac{20}{20}$ is not incompatible with *annoying astigmatism, the writings of specialists to the contrary notwithstanding.*

The usual history of such students is, that up to within a short period, they had perfect control of their eyes, and were in the habit of reading all day and half of the night, with only short intervals of rest. Recently when preparing for an examination, which called for an extra amount of application, their eyes have given way. Now they cannot read for ten minutes without pain and headache.

With a great many of these students the continuous use of a 0.25 cylinder, usually a concave lens, set horizontally, will promptly restore all suspended privileges, and in a very few days enable full work to be resumed. Experience also teaches the wearer of these spectacles, that as long as hard work is continued these glasses cannot be dispensed with, even for a moment, without the return of eye and head pains.

It is very interesting to witness how rapidly the reflex irritations subside, when the irregular movements of the accomodating muscles are no longer called into play. Persons who could not look out into the sunshine before the consultation, find the weak cylinders and their restfulness more comfortable than the smoked glasses which they had been wearing.

It is also an experiment of very great interest in such small degrees of astigmatism as 0.25 cylinder will correct, the more especially if the angle of the fault be an oblique meridian, to note how the reversing of the lens, say from 30° to 150°, will instantly bring on eye pains and head confusions, and they become so uncomfortable that the patient cannot stand them. Then note the expressions of satisfaction when the lenses are replaced in their proper position. This experiment alone will attest the influence which the very weak cylinders exercise towards the comforting of aching eyes and heads. Even during the office consultation a difference is felt in loss of comfort as soon as the cylinder glasses are removed from the eyes.

I find myself frequently discarding + spherical glasses given for the correction of muscular accommodative astheropia, because with V.=$\frac{20}{20}$ the true cause of the painful eyes and head aches, which had continued, notwithstanding the use of the + s., *a low degree of astigmatism*, had not been found out, or had not been considered worthy of recognition by the specialist who preceded me in the treatment of the case.

I also find myself frequently substituting the 0.25 cylinder for the 0.5, which is the weakest lens that many ophthalmic surgeons prescribe. Patients who cannot use their eyes for even a few minutes with the 0.5 — c., can read by the hour with the 0.25 c. When these patients are examined by the clock dial lines they will promptly discard the 0.5 in favor of the 0.25 in the uniform coloring of the lines.

Knowing by my every day experience, the value of the 0.25 cylinder lenses, also the great annoyance which this low degree of astigmatism produces under eye pressure, and being therefore constantly on the lookout for it, I can nearly formulate this proposition. Patients with V.=$\frac{20}{20}$, reading readily the finest print, with distant vision not much improved by + or — glasses, persons who cannot use their eyes for near work without pain, will be found on careful examination to have a low degree of astigmatism which a 0.25 cylinder will correct, to their very great comfort.

Often in testing these eyes which pain on use, they seem to possess perfect vision. In many the astigmatic dial will appear correct, all the lines seeming to be of uniform blackness. These are the patients to whom + spherical glasses are given by many ophthalmic surgeons, because the examination is deemed complete, and the low degree of astigmatism has not been detected. Put a 0.25 spherical lens before one eye, the other being closed, and the difference in the coloring of the lines will at once become prominent. *The latent, but painful astigmatism, becomes apparent.*

It is very important in the detection of these low degrees of astigmatism to close one eye. At times I find the error of refraction in one eye at right angles to the faulty meridian of the other. With both eyes open the clock dial lines are perfect, each eye seemingly correcting the fault of the other. Should the patient be examined with

the two eyes directed to the astigmatic lines the cause of painful vision would remain unnoticed. It is only when one eye is closed that the fault becomes recognizable.

Even with the one eye the sharpness of outlines of the radiating lines will vary. First one, then another, seems better outlined as the ciliary muscle modifies the crystaline lens surface in its attempt at overcoming the fault in the cornea. After staring at the dial for some little time the true meridian fixes the best lines. The faulty lines do not always come out in gray, contrasting with the sharply cut black ones. Sometimes they appear as bright blue or red, and startle the patient by the contrast,

In correcting these weak degrees of astigmatism I find the use of atropia very misleading. Hence, as a rule, I do not use it in determining the kind, and the direction of the faulty focusing in cases of V.$=\frac{20}{20}$, unless glasses prescribed without the use of atropia, do not give the desired or expected comfort. When I speak of atropia I mean homatropia, which I have exclusively used for many years for refractive work. With very few exceptions the mydriatic reverses the kind and the direction of the faulty meridian. An astigmatism of 0.25 — c. 0. becomes under atropia 0.25 + c. 90. In the majority of patients the reversed lens + c. does not give the degree of comfort in its use that the — c. will, the one which corresponded with the normal condition of the eye, before the atropia was used. I find these low degrees of astigmatism, as modified by atropia, a fictitious condition which will most frequently mislead the surgeon who accepts it. Should there be any doubt in the mind of the patient as to the exact angle of the fault, then atropia lends a valuable aid in determining it.

As this, however, is not intended for a paper on astigmatism, an important error of refraction, but is designed to call attention to the necessity of correcting the minor degrees of irregular refraction in patients who have V.$=\frac{20}{20}$, a condition which many ophthalmic surgeons ignore, my object can be best illustrated by giving a synopsis of my individual office work for 1888. This list refers to private practice only, and does not include the many cases of astigmatism, worked out by my assistants at the Presbyterian Eye and Ear Charity Hospital where my dispensary work is done. In my office case-book for the year 1887, I have recorded the history of 493 persons for whom I have prescribed cylinder glasses. As in many of these persons each eye differed both as to the kind, the degree and the angle of the irregular refraction, I have thought it best in tabulating my astigmatic work, to refer to the 986 eyes rather than to the 493 persons. All of these patients sought advice for the relief of eyes which were painful on use. I used atropia on 135 of the 493 persons, in about 27 per cent. of the cases. These were pay patients who expected relief. Had the glasses prescribed not given comfort, they would have returned for further treatment, and I would then have used atropia to sustain, correct, or to modify my first diagnosis. I must therefore believe that the majority of the glasses prescribed, without the use of homatropia instillations, were satisfactory. In a great many instances I have evidence of the entire comfort secured, and the perfect control regained over the eyes even months after the glasses 0.25.— c. were given.

TABLE *Showing the Direction of the Error of Refraction.*

No.	Angle.
267	— 0.25°.
90	— 0.25⁹⁰.
79	— 0.25 oblique.
29	+ 0.25°.
85	+ 0.25⁹⁰.
16	+ 0.25 oblique.
63	— 0.5°.
23	— 0.5⁹⁰.
37	— 0.5 oblique.
6	+ 0.5°.
28	+ 0.5⁹⁰.
14	+ 0.5 oblique.
23	— 0.75°.
9	— 0.75⁹⁰.
15	— 0.75 oblique.
4	+ 0.75°.
10	+ 0.75⁹⁰.
7	+ 0.75 oblique.
15	— 1.°.
6	— 1.⁹⁰.
15	— 1. oblique.
1	+ 1.°.
13	+ 1.⁹⁰.
10	+ 1. oblique.
9	— 1.25°.
5	— 1.25⁹⁰.
12	— 1.25 oblique.
0	+ 1.25°.
5	+ 1.25⁹⁰.
4	+ 1.25 oblique.

TABLE *Showing the Degree of Astigmatism in 986 Eyes Prescribed for in the Office Practic of Dr. J. J. Chisolm, of Baltimore, Md., during the year 1887.*

No.	Deg.
546	0.25. D.
171	0.5. D.
66	0.75. D.
60	1. . D.
35	1.25. D.
22	1.5. D.
10	1.75. D.
27	2. . D.
8	2.25. D.
11	2.5. D.
5	2.75. D.
12	3. . D.
6	3.5. D.
2	4. . D.
1	5. . D.
1	5.5. D.
3	6. . D.

Of the 986 eyes, all of them more or less painful in use, so that relief was sought, 546 indicated an error of refraction which a 0.25 — cylinder corrected. Had I not sought for, and

found this small error, which in the relief given proved to be the cause of all discomfort, I would have turned away nearly two-thirds of my patients unrelieved. Some of these patients brought painful eyes and 0.5 cylinders prescribed by ophthalmic surgeons of deservedly high standing. The change from the 0.5 + or — cylinder lens which over corrected, to the 0.25 cylinder which made the lines on the dial uniform, removed at once the discomfort complained of. It enabled patients to read by the hour when with the stronger glasses they could not use their eyes at all. This magical and immediate change was brought about by no other treatment than the recognition of the low degree of astigmatism, and the substitution of 0.25 for 0.5 cylinder. In some cases of defective vision sight would be improved from $\frac{1}{3}\frac{5}{0}$ to $\frac{1}{1}\frac{5}{5}$ by these weak cylinders.

As I have been pursuing this line of investigation for several years, and have learned to suspect low degrees of astigmatism in eyes painful in use, which exhibited V.$=\frac{2}{2}\frac{0}{0}$, in seeking for it I find it. I could readily, from my case-book, give the detailed account of hundreds of patients, who, from not being able to read for more than a few minutes without glasses, could by the aid of a 0.25 cylinder, read for many hours.

It is only necessary to call attention to the frequency, and to the annoyance of these small degrees of astigmatism to have others find them as frequently as I do. The more familiar one becomes with these minor defects the more readily are they detected. I am sure that every month's experience adds to my facility, and also to the number of cases seeking advice. For the year 1887, 546 painful eyes showing only 0.25 D. fault were prescribed for in my private practice, and were relieved.

Through this paper I desire to impress others with my conviction, that painful eyes with V.$=\frac{2}{2}\frac{0}{0}$ very frequently means astigmatism. Also that experience will show, that the most useful cylinder for the correction of eye and head pains in this very large number of patients, is the 0.25 D. cylinder. From my standpoint it becomes the most valuable astigmatic lens of the Trial Case.

DR. H. CULBERTSON: As to the 0.25 diopter being the more useful of lenses in the correction of ametropia, I can verify Dr. Chisholm's position to the letter. Not infrequently I can double vision with a D. 0.25 + or negative. As to the correction of ametropia upon the apparent refraction of an eye, I would say that I have not infrequently been compelled to do this after having used mydriatics and when the latter denoted the employment of different glasses.

As to binocular astigmatism, I must assure the Section, and especially Dr. Savage, that my views are original on this subject, and that I had not seen his article on this theme until he kindly presented me with a copy since I read my paper before the session. The doctor and myself have both been engaged in investigating this form of ametropia for about the same length of time and independently and unknown to each other.

Being a devotee to truth and not wedded to any solution of the *modus operandi* of binocular astigmatism, or claiming in my paper more than a hypothesis as to the cause of this anomaly, I can only learn with pleasure of the views of Dr. Savage. In looking over his brochure on this subject hastily I understand that gentleman to hold that the entire phenomena of binocular astigmatism is due to anomalies of the four oblique muscles. If I am correct in this inference, I am compelled to differ on this point, believing that the state of the recti are concerned in this anomaly not infrequently, as well as the oblique. The five-minute rule will not permit me to enlarge upon my views on this point.

As to the relation of the ciliary muscle to this anomaly I cannot now state a positive opinion, for my investigations as yet do not permit me to say that it has not a positive or negative influence in the phenomena of binocular astigmatism. As for myself, future investigations must determine this point.

SOME OF THE INFLUENCES OF THE SYMPATHETIC NERVOUS SYSTEM IN DISEASE.

Read in the Section on Practice of Medicine at the Thirty-ninth Annual Meeting of the American Medical Association, Cincinnati, May, 1888.

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My attention was drawn to the influences of the sympathetic nervous system, in diseases of mucous membrane, in connection with my studies in rhinology and laryngology. In a paper read before the fourth annual meeting of the American Rhinological Association, at St. Louis, in 1886, on "Naso-pharyngeal Catarrh, as a Cause of Neurasthenia," I attempted to show that functional derangement of the different portions of the body were effected through the great sympathetic nervous system. Also, in a paper read before the American Rhinological Association, at Washington, last September, on "The Pathology and Treatment of Naso-pharyngeal Catarrh," in which I attempted to demonstrate that the diseases of the nasal and pharyngeal mucous membrane, generally called naso-pharyngeal catarrh, was not an inflammation of the mucous membrane, but was a condition produced by paresis of the sympathetic ganglia and nerves supplying the mucous membrane and blood-vessels of these parts.

In the last mentioned paper I alluded to the

fact that if paresis of the cerebral and cervical ganglia and nerves of the sympathetic nervous system caused catarrhal and other troubles in the upper air passages, that it might be the means of explaining a great many pathological conditions in organs and portions of the body supplied by the sympathetic nervous system. It is for the purpose of calling attention to the pathological conditions of the sympathetic nervous system, or to the pathological conditions of the parts supplied by this system of nerves, that I present this paper. The term pathology is too frequently used where pathological anatomy should be used. In a large number of troubles produced by paresis of the sympathetic system, we are not able to detect anatomical lesions. Pathology bears the same relation to pathological anatomy that physiology does to histology.

I will not take up your time with the anatomy of this great nervous system, taking it for granted that every member of the American Medical Association is familiar with the anatomy and histology of both the cerebro-spinal and sympathetic nervous systems.

The anatomy, histology, physiology and pathology of the sympathetic nervous system is one of the most interesting and of the greatest importance to every medical man. Yet it is a subject that is very much neglected in our medical colleges and by medical men after they enter practice.

This system of nerves is sometimes called the ganglionic nervous system, and the nervous system of organic, or vegetable life. By a more thorough knowledge of the sympathetic system we might be better able to understand what appears to be some of the mysterious phenomena of disease.

Commencing as it does within the cranium by the ophthalmic ganglia, with its double chain extending down on either side of the spinal column, terminating below in front of the coccyx in a single ganglion—the coccygeal ganglion.

In the cranium we have four ganglia, called the cerebral ganglia, the ophthalmic, the sphenopalatine, the otic, and the sub-maxillary. In the neck there are three cervical ganglia; the superior, middle, and the inferior. In the chest are situated the twelve thoracic ganglia, corresponding to the twelve ribs. In the abdomen we find the great semilunar ganglia, sometimes called the abdominal brain, with its large plexus of nerves, called the cœliac-plexus. In the lumbar region, in front of the spinal column, are the four or five lumbar ganglia. In the pelvis we have the four or five sacral or pelvic ganglia, and terminating below and in front of the coccyx, is a small single ganglian, the last of the double chain called coccygeal ganglion, or ganglion impar. Thus the sympathetic nervous system, or cord, as it is sometimes called, consists of from twenty-eight to thirty ganglia on either side of the spinal column, terminating below in a single ganglion. Each ganglion is connected by nerves with the other ganglion of the same portion, each group of ganglia being connected with the group directly above or below. Each ganglion is also connected with the cerebro-spinal nervous system, receiving filaments from the cerebro-spinal afferent nerves and giving off nerves to the cerebro-spinal efferent nerves in addition to the filaments it gives off to the organ or tissue it supplies.

The question has frequently been asked as to the dependence of the cerebro-spinal and sympathetic nervous system upon each other. In a course of lectures recently delivered by Dr. Gaskell, before the Royal Institute of London, he claims that no new nerve fibres arise in the sympathetic ganglia, but that the fibres destined to them from the cord comes from the inner portion of the posterior horns and inner portion of the gray matter. They are medullated, but in passing through the ganglia they lose their medullary sheath and divide into smaller strands. The old distinction between the two nervous systems, that the cerebro-spinal needs repose, whilst the sympathetic does not, since it cannot be proved, and indeed, is highly improbable that the activity of the brain and spinal cord is ever completely in abeyance. Dr. E. Long-Fox, in his excellent work on the sympathetic nervous system, gives a number of instances of the natural dependence of the two systems. He says, "The fact, that in spite of this natural dependence the sympathetic, under conditions of lesions of brain or cord, is enabled within certain limits to act independently." Parks states that nutrition is probably carried on with complete destruction of the cerebro-spinal centres.

Fox states that "reflex irritation of the vasomotor nerves can be entirely limited to the particular organ or tissue supplied." Thus in Vulpian's experiment, some days after the transverse section of the sciatic nerve, or of the brachial plexus, when the corresponding pulp of the paws of the animals had become quite pale and anæmic, he was able by slight rubbing of these pulps to cause a reflex congestion. The fact of the continuance of the heart's action for a time in some mammals after its separation from the body, in the frog, after the destruction of the brain and medulla oblongata, irritation will cause congestion of the limbs. In mammals, after section of the cord at the mid-dorsal region, sensory excitation of one posterior limb will cause reflex heart phenomena in the other.

While the sympathetic ganglia may not, and probably do not, have the power of generating nerve force independently of the cerebro-spinal system for any great length of time they do seem to act as storage batteries and accumulate nerve force generated by the cerebro-spinal system.

This accumulated force is capable of carrying on the life process after the cerebro-spinal system ceases to act. But this action can only be kept up for a limited time unless reinforcement is received from the cerebro-spinal. This reinforcement is received through the afferent filaments going to each ganglion. If a lesion of the cerebro-spinal system takes place in the region of these afferent nerves, or the afferent nerve is severed, the ganglion will only retain its power for a limited time and the same result will take place that is found after the lesion of the ganglia or its nerves.

Fox says, "Lesions of the cervical cord may cause the same oculo-pupillary phenomena as lesions of the superior cervical ganglion." Lesions of the cervical cord (seventh cervical and first dorsal) may cause the same vaso-motor paralysis evidenced by unilateral vascular dilatation of the face, ear, and head, as is seen in lesions of the cervical sympathetic ganglion.

Hemisection of the lower third of the dorsal region of the cord produces vascular dilatation in the lower limit of the corresponding side, and so increase of temperature.

All lesions of the spinal cord, and pressure on it, may enfeeble the vascular tone of the parts in relation by their vaso-motor nerves with the region of the cord below the lesion. The general distribution of the sympathetic nerves is to the mucous membrane, and possibly to integument, to non-striated muscular fibres to the heart, and particularly to the muscular coats of the arteries. The parts principally supplied with sympathetic nerves are usually capable of none but involuntary movements, and when the mind acts on them at all it is only through the strong excitement or depressing influence of some passion, or through some voluntary movement with which the action of the involuntary part are commonly associated. In the volume on the nervous system in "Flint's Physiology of Man," we find a long list of experiments made upon the sympathetic system, for which I am much indebted, as I make use of a number of them.

In 1727 Petit made some experiments upon the cervical portion of the great sympathetic nervous system. His experiments only showed its influence upon the eye, also, that the influence of the sympathetic nerves was propagated from below upwards towards the head, and not from the brain downward, as in the cerebro-spinal system.

In 1816 Depuy removed the superior cervical ganglia in horses with effect of producing infection of the conjunctiva, increase of temperature in the ear, and abundant secretion of sweat upon one side of the head and neck. In one experiment upon the removal of the ganglia on both sides, in a horse already feeble and emaciated, the face and ears became hot and moist. He says that "the consequences of destruction of the ganglia are, contraction of the pupils, redness of the conjunctiva, general emaciation, as well as œdema of the extremities, and a general cutaneous eruption. These experiments show that the sympathetic has an important influence on the circulation, nutrition, calorification and secretion.

In 1851 Bernard repeated the experiments of Petit, dividing the sympathetic in the neck on one side, in rabbits, and noted on the corresponding side of the head and ear increased vascularity and an elevation in temperature amounting to from 70° to 110° F. This condition of increased heat and vascularity continued for several months after division of the nerve. In 1852 Brown-Séquard repeated these experiments and attributed the elevation of temperature directly to an increase in the supply of blood to the parts affected. He made a most important advance in the history of the sympathetic, by demonstrating that its section paralyzed the muscular walls of the arteries, and further, that galvanization of the nerve in the neck caused the vessels to contract. This was the discovery of the vaso-motor nerves.

The experiments of Petit, Bernard, and of Brown-Séquard, were of great value, as they demonstrated that the sympathetic influences the general process of nutrition, and that many of its filaments are distributed to the muscular coat of the blood-vessels. Flint says, "when the sympathetic is divided in the neck the local increase in temperature is always attended with a very great increase in the supply of blood to the side of the head corresponding to the section. The increased temperature is due to a local exaggeration of the nutritive processes apparently dependent directly upon the hyperæmia. There are numerous instances in pathology of local increase in temperature attending increased supply of blood to restricted parts."

If the sympathetic is divided in the neck of a rabbit and both ears are cut off just above the head with a sharp knife the artery on the side on which the sympathetic has been divided will frequently send up a jet of blood to the height of several feet, while on the sound side the jet is always much less forcible, and may not be observed at all.

Analogous phenomena have been observed by section of the sympathetic in other parts of the body. Samuel has noticed an intense hyperæmia of the mucous membrane of the stomach and intestines following extirpation of the cœliac-plexus. By comparative experiments it was shown that this did not result from the peritonitis produced by the operation.

When the sympathetic filaments distributed to a gland are divided the supply of blood is very much increased, and an abundant flow of the secretion follows. Dr. Moreau has made a series of observations on the influences of the sympathetic nerves upon secretion of liquid by the intestinal canal, which are particularly interesting in their

bearing upon the sudden occurrence of watery diarrhœa. In these experiments the abdomen was opened in a fasting animal and three loops of intestines, each from four to eight inches long, were isolated by two ligatures. All the nerves going to the middle loop were divided, care being taken not to sever the blood-vessels. The intestines were then replaced and the wound in the abdomen was closed with sutures. The next day the animal was killed. The two loops with the nerves intact were found empty, as is normal in fasting animals, and the mucous membrane was dry, but the loop with the nerves divided was found filled with a clear, alkaline liquid, colorless, or slightly opaline, which precipitated a few flocculi of organic matter on boiling. I will not enter into the details of the function of the sympathetic nervous system. It is well established that it regulates the blood supply, the temperature, the nutrition and secretion. And in the experiments mentioned it has been seen that the destruction or removal of any ganglia, or section of the sympathetic nerves supplying a certain part or organ of the body, alters the circulation, temperature, nutrition, or secretion, or all these, in the part of the body or organ which receives nervous supply from that portion of the sympathetic nervous system. It is thought that the sympathetic has the power both of conducting, transferring, reflecting, and possibly of augmenting or of inhibiting impressions made on it. Near the terminal filaments of the sympathetic nerves there exists numerous ganglionic cells.

The marvelous effects of reflex action in health and disease, in connection with the sympathetic nervous system, is well recognized by every medical man. For coarse stimuli the spinal cord and medulla oblongata are the chief centres for vasomotor reflex action. But the heart is more or less independent of these great nerve centres. A reflex arc exists in its own substance. Reflex movements are excited from all sensory nerves, not only spinal, but also sympathetic. Claude Bernard has shown that reflex action can occur in the sub-maxillary gland when all nerves that communicate with the cerebro-spinal centre are cut. If the superior cervical ganglia are separated from the higher nervous centres oculo-pupillary phenomena can be reflexly excited. That the uterine centres can act independently is seen in the occasional expulsion of the child after the death of the mother. The expulsion of fæces *per anum* after the death of the patient shows also that the sympathetic ganglia of the intestines are centres for independent reflex acts.

In a paper read by Dr. Woakes before the International Medical Congress, at London, he speaks of the sympathetic ganglia as reflex centres. Drs. Fox and Woakes give a number of examples to show the reflex power residing within the sympathetic system.

It is not my object in this paper to call attention to the reflex phenomena of the sympathetic system, but apply the physiological facts we have mentioned to the influence of this system of nerves upon the circulation of the blood and the condition of the blood-vessels, more particularly to that condition called catarrh, or inflammation of mucous membrane, and to the deranged function of certain organs dependent upon disturbance of local circulation. It is impossible to give a definition of inflammation that will be brief and exact at the same time. Inflammation is a term implying a whole series of processes, partly vascular, and partly textural, and their processes admit of great varieties of combinations. We have certain characteristics of inflammation but the whole content of the term cannot be fully indicated without describing briefly the process to which the term is applied. Four cardinal symptoms of inflammation are well recognized, namely: *redness*, *swelling*, *pain* and *heat*, with impaired function. The *redness* is from increased afflux of blood to the part. The *swelling* to an increased volume of blood in the part, and from exudation. The *pain* is caused by the swollen condition of the parts, which cause pressure upon the sensory nerve filament, or by chemical irritation. The *heat* comes from the excessive flow of blood through the part in the stage of hyperæmia.

In inflammation we have both vascular and textural changes. The first vascular change is produced by some *irritant* which being applied there is a general dilatation of the vessels, first of the arteries, then of the capillaries and veins. The flow of blood through the widened channels is more rapid at first. This is the stage of hyperæmia. After a time the speed diminishes, and at length the flow becomes slower than in the normal condition. This constitutes the stage of congestion. During this stage we have a migration of the blood corpuscles through the walls of the veins and capillaries into the surrounding tissue, but not from the arteries associated with the passage of the corpuscles. There is always an escape of liquid, which is comparatively rich in albumen. This is the stage of exudation, or infiltration.

All these changes and stages depend upon a molecular alteration in the walls of the blood-vessels. Mere paralytic dilation does not give rise to slowing of the current or to the passage of the blood corpuscles through the walls of the vessels. The inflammatory changes in the blood-vessels must of necessity be associated with tissue changes. These changes vary with the nature of the exciting cause, and with the intensity of the inflammation, with the character and extent of the vascular disturbance, and with the nature of the tissue. Inflammation cannot exist without molecular death.

Is it possible to have these cardinal symptoms of inflammation, *redness*, *swelling*, *pain* and *heat*,

with impaired function dependent upon paresis, or paralysis of the sympathetic nervous system, without inflammation, without structural change in the blood-vessels, without molecular death? This question opens up the question of contractions, dilatation, erectility and reflex vaso-motor effects. The function of the vaso-motor nerves most frequently called into exercise is contraction.

Fox says, "The motor nerves that preside over the muscular contraction of vessels, and rule the local circulation, are the nerves that issue mainly from the ganglia of the great sympathetic, creep along the arterial walls, and can be followed into the middle muscular coat of the arteries. The vaso-motor apparatus therefore is in a state of permanent activity, never in repose, never inert. The muscular tunic of the vessels is in a state of semi-contraction—in other words, of vascular tone. Variation in this tone will be the necessary consequence of various modifications of the nervous apparatus."

The vaso-dilators are also in a constant state of action but are fewer in number than the contractors. Were it not for the pressure and action of the vaso-dilators, the vaso-contractors would cause undue and complete contraction of the vessels. The arteries possess both elasticity and contractility; the small arterialis contractility only. The vaso-contractors and dilators acting together keep up the proper calibre of the vessels.

When from the presence of some irritant upon the vaso-motor nerves, or as the result of mental impressions, etc., by reflex action, the vaso-motor nerves become affected, lose their tone, the ganglia soon loses its power, paresis, or partial loss of power takes place, the vessels dilate and remain dilated. As the result of this ganglionic paresis, we have *redness* from the increased afflux of blood to the part, we have *swelling* from increased amount of blood in the part, and after some time from the excessive nutrition furnished the tissue, the *pain* being produced by the pressure upon the sensory nerves, the *heat* from the rapid and excessive flow of blood through the part, we do not have changes taking place in the vascular walls. In the experiments made by Petit, Bernard, Brown-Séquard, and others, to which allusion has been made in this paper, it was shown that by removal of the ganglia, or section of its nerve filaments, that the part of the body supplied from that ganglian had an increased flow of blood into and through the part. That there was considerable increase of temperature, *redness* and *swelling* of the part, and, no doubt, *pain*. That there was increased secretion and nutrition.

In conclusion. When we take into consideration the anatomy and distribution of the sympathetic nervous system, together with its physiological functions, and its pathological action under artificial impairment, is it not reasonable to suppose that if a ganglion becomes impaired and loses its power that we might have such conditions taking place that would explain a number of the phenomena occurring in diseases of organs or other parts of the body supplied by the great sympathetic nervous system?

Would it not explain many of the functional brain troubles? of eye, ear and nose, pharyngeal and laryngeal troubles. Pneumonia can be more easily explained in this way than in any other. Some heart, stomach, and kidney troubles can be explained by sympathetic paresis. Gynecological troubles lose half their terror when viewed from this light.

We all are aware that in the cases of troubles mentioned recovery is more rapid and complete when treatment is directed to the sympathetic nervous system.

A PLEA FOR THE BETTER RECOGNITION OF THE OCULIST IN THE SERVICE OF THE U. S. PENSION DEPARTMENT.

Read in the Section on Ophthalmology, Otology and Laryngology, at the Thirty-Ninth Annual Meeting of the American Medical Association, May, 1888.

BY JOHN W. WRIGHT, M.D.,
OF COLUMBUS, OHIO.

The U. S. Pension Department has been established for the purpose of remunerating those who have become disabled while in the service of their country by reason of injury, sickness or deprivation.

While the Government is endeavoring to provide for these persons, it becomes necessary to guard against impostors, for the recompense is amply adequate to induce those who are so disposed to make the effort to obtain it. For this reason boards of examining surgeons and special agencies have been established throughout the country, as well to enable the deserving to procure what is justly due them as to prevent others from obtaining that which does not belong to them.

At present the applications for this beneficence are in great profusion, for the reason, perhaps, that many of the older ones who served in the late civil war mistake their physical impairments from age for disabilities from wounds and exposures from army life; while others who received injuries while in service or suffered from the sicknesses and privations consequent to a camp life, although not considered at the onset sufficiently disabled for a pension, have developed with increasing years diseases and infirmities which have rendered them confirmed invalids.

It is doubtless the design of the Government authorities that the machinery of the Pension Bureau shall be as complete as possible; for the nearer it approaches perfection the more will the

chances for fraud be diminished, hence every application receives the most careful and deliberate consideration at the hands of this department.

The Medical Referee, in his instructions to examining surgeons, states that (Art. 5) "the object of a medical examination is to obtain a full and complete description of the disabilities for which pensions are claimed, and to gain a *clear and distinct idea of the extent to which they disable in the performance of manual labor.*" (Art. 6.) "To this end it is necessary that *every disability existing* shall be fully described," etc.

Under the heading of "*Diseases of the Eyes,*" (Art. 58) we find: "In all diseases of the eyes involving internal structures the ophthalmoscope must be brought into use, otherwise the examination is of little value."

In closing the instructions the medical examiners are reminded "that the interests involved in pensions are of such magnitude as to demand the employment of the very best skill this country can supply." The term "a full and complete description of the disabilities" is sufficiently significant to indicate the desire of the head of the medical department.

That "the ophthalmoscope must be brought into use, otherwise the examination is of little importance," is pertinent; but could the department have fully comprehended the necessary knowledge of optical principles and the constant and unremitting practice which underlie a correct application of this instrument, in order to be able to detect the various pathological conditions in eye affections, I do not believe this last clause of instructions would have been imposed upon medical gentlemen of whom not one in every hundred can realize the practical value of this instrument. This may seem to many an exaggeration, yet I believe every oculist here will bear me out in this assertion. Nor is this intended to reflect upon the abilities of examining surgeons—for there is not an instrument of diagnosis of such incalculable value, in my estimation, so difficult of application as the ophthalmoscope.

In making this entreaty, a few cases which have come under my immediate observation may be in place.

A gentleman living in the eastern part of Ohio, who served his country during the War, made an application for a pension because of a disease of the eyes which rendered him almost blind and incapable of earning a livelihood.

He was ordered before a board of examiners, and in due time he received a pension and considerable back pay. Some time afterward he was induced by his friends to consult me in reference to his eyes. I found that he had entropium, which had followed what he termed the "army sore eyes." There was no complication or structural disorganization of the parts, the inflammation being simply mechanical, the result of the hairs which were constantly rubbing against the balls.

I informed him what could probably be done and, seeing an opportunity of giving him great relief, I encouraged him with the possibility of considerable improvement in his vision. I saw that he was not favorably impressed with my opinion, and I naturally presumed that he did not have the confidence in my ability to accomplish that which I had given him encouragement could likely be done. I afterward learned that he had *too much* confidence in my ability to help him. He feared that the operation would prove so successful that he would be compelled to relinquish his pension. Afterward, upon making inquiry, I was surprised to learn that he had obtained his pension because of "glaucoma."

Mr. B. had served considerable time in the army. Four years ago I operated upon one of his eyes for cataract, he being blind in both eyes. The operation proved very successful and the patient returned to his former occupation—that of a contractor—which he had been compelled to relinquish almost a year before the operation, and which he now manages with very little inconvenience.

Not long since he got into the hands of a pension solicitor and was easily made to believe that his blindness had been the result of sickness and exposure while he was in the service. He "*did* remember of having had the jaundice while in camp," and how "*yellow*" his eyes looked. "This was doubtless the key to his blindness."

An application for a pension was at once made, and in due course of time he was ordered to appear before the board of examining surgeons. One of the examiners who makes some pretensions to a knowledge of eye diseases and the use of the ophthalmoscope was delegated to examine all such applicants, and accordingly examined this gentleman's eyes.

An elaborate examination was at once commenced, and the ophthalmoscope was brought into vigorous use. An hour at least was consumed in the examination. Of course the opaque lens was seen in the eye which had not been operated upon, or I presume it was, as the ophthalmoscope was made to play upon it at least one-half of the time; and in the other eye the cicatrix where the incision had been made in the upper part of the cornea, and the irregular-shaped pupil, were plainly discernible, and excited the interest of the surgeon.

The surgeon inquired what had happened to the eye. Evasively he was answered that "some doctor had been trying to operate upon it." The examining surgeon then ventured the remark that it had been "*badly butchered,*" and advised him not to allow the other eye to be "meddled with," or he would be left "*entirely blind.*" The applicant has received, I understand, a comfortable pension.

Of course it is impossible for us to say that the applicant's jaundice, together with his other sicknesses and exposures in the service, *did not* cause or contribute in some way to the opaque condition of his lenses, although his mother and a sister, who were not exposed to these influences, were both blind from cataract.

Although the origin of a disability is considered a legal matter; that is, a thing to be determined by evidence obtained outside of the medical examination, yet the examining boards are largely entrusted with the duty of distinguishing and separating disabilities of origin either before or after enlistment from those which occurred during the actual service.

In the above case the opinion of any reputable oculist, doubtless, would have precluded a pension.

A gentleman from the southern part of Ohio came to my office a few years ago and desired me to make an examination of his eyes and give him a written opinion. He made the statement that while in the army he had had sunstroke, and with it the loss of an eye. He was discharged and sent home, not on account of the loss of the eye, but because of his general physical disability which followed the sunstroke.

A few months after returning home, while engaged in some work requiring physical exertion, he suddenly lost his sight in the other eye. He was now practically blind, although not *entirely* so, for he could grope his way around to a better advantage than if he had been totally blind, but he was so blind that it was impossible for him to distinguish one object from another, or to be able to perform any labor that could not have been accomplished by a blind man. He had made several attempts to procure a pension, and had been ordered before different examining boards; but for some unaccountable reason to him he had never been able to obtain it. Finally it came to his ears that it was intimated by some persons that he was a malingerer, and it was to contradict this report and to place himself in a proper standing among his friends, rather than an aid in procuring the pension, that he desired the examination and a written statement concerning the condition of his eyes.

The examination with the ophthalmoscope revealed the fact that there had undoubtedly been very extensive retinal hæmorrhages involving the maculæ in both eyes. The hæmorrhagic spots had been unquestionably very numerous, as there were many places marked by choroidal alteration or replaced with blackish pigment. In other places the retinal tissue seemed to be degenerated and marked by a whitish or grayish tinge. There was such complete destruction of the retinal tissue that it was impossible for him to more than know that an object was in front of him in good light. I gave him a certificate stating the facts, since which time I have heard nothing from him.

I do not recollect of ever having seen or heard of retinal hæmorrhage caused from sunstroke, but, owing to the engorged condition of the sinuses and veins of the brain during the active stage of the affection, it is readily understood how it could occur.

The second hæmorrhagic attack can be explained because of the tendency of recurrences of cerebral engorgement after sunstroke, marked by sudden depressions upon exposure to heat or bodily exercise.

It is fair to presume that the surgeons who had examined him took into consideration the external appearances of his eyes, and had overlooked their retinal condition.

As oculists are generally aurists, the following case which refers to the hearing is permissible:

A young man enlisted at the beginning of the War and was in active service most all the time until its close. He was in many engagements where there was heavy cannonading. When he was discharged he was somewhat deaf, and grew gradually worse for ten years following, at which time it was almost impossible for him to understand the loudest talking. Since that time he has remained in this condition.

About eight years ago I saw him, when he was making an effort to obtain a pension. He came to me because I had known him before he entered the service, and also because, he said, he had consulted me in reference to his deafness immediately after his return from the army. I have, however, no record or remembrance of his consulting me at the time mentioned by him, but from an incident which he referred to as having occurred at the time, I presume he did. At any rate I was not able to testify, as he desired evidence to establish the time the deafness began and its continuance, in order to associate it with the exposure to the heavy cannonading during the service.

Having no record of the examination I made eight years ago, I am not prepared to give the details, yet I am confident I found no appreciable lesion, because of an opinion I formed of his case at the time, and which I have had in my mind ever since, relative to the cause of his deafness.

I came to the conclusion that there was an anchylosis of the joints of the bones of his ears. These bones, as we all know, are, or should be, freely movable at their joints, although delicately attached to each other. Their articulating surfaces are covered with cartilage and connected with capsular ligaments, and lined with synovial membrane, the same as the articulating surfaces of other bones, and are no doubt liable to the same diseases as the larger joints. These joints are so adapted as to allow of the motions of the bones conveyed to them through the membrana tympani from ordinary sounds, and they perform their functions perfectly well and without injury to themselves under these conditions. But heavy

cannonading would cause, through this membrane, excessive action of these joints. Then why is not a synovitis just as likely to be induced here as it would be in the larger joints, where it is so liable to end in an anchylosis?

But it matters not whether the deafness is caused from an anchylosis of the bones of the ear or, as some authors would have it, in case of boilermakers and others subject to like noises, that it is due to nervous exhaustion from continued shock, or, as explained by others, from chronic catarrh of the middle ear; it is quite sufficient for us to know that cannonading has been the cause of very much and intense deafness.

In this case it is impossible, perhaps, to establish an unbroken chain of evidence from the time of the inception of the injury until very marked deafness ensued, from the very fact that slight deafness, or what is termed dulness of hearing, is not very noticeable except to one's most intimate friends or associates. When the patient is no longer able to distinguish ordinary sounds, then it is noticeable to all.

Evidence that this man was exposed to heavy cannonading during his service, and that the deafness now exists—in the absence of lesions that would indicate that the deafness was not the result of army exposure—would be sufficient grounds for allowing him a pension.

If, as expressed by the Medical Referee, "the interests involved in pensions are of such magnitude as to demand the employment of the very best skill the country can supply," then it is nothing but just that *all* applicants claiming defect in sight or hearing should be referred to boards of examiners composed solely of experts in this particular class of affections.

The number of applicants claiming defect in vision alone would justify this. From July, 1886, to December, 1887, there were 4,672 pensions granted for disabilities of the eyes. As about 12 per cent. of all applicants during this time received pensions, then there was the enormous number of 39,000 applications in this class alone.

In concluding this argument I disclaim any selfish motives more than a desire that our profession may be the means of promoting the interests of many worthy applicants, and of increasing the efficiency of the Pension Department by contributing the very best skill for the examination of this particular class of applicants.

OXYPIPERIDIN, a substance derived from the distillation of benzolamidovalerianic acid, is very similar to strychnine in its action, being a strong tetanizer. The crystals of oxypiperidin melt at 30°–40° C. They are soluble in water, alcohol, ether and dilute acids. When oxypiperidin is boiled with strong acids the non-toxic substance α-amidovalerianic acid is formed.

A CASE OF PLACENTA PRÆVIA.

Read before the Medical Society of the District of Columbia, April 25, 1888.

BY PHILIP MARVEL, M.D.,
OF WASHINGTON, D. C.

The subject of this report, Mrs. S., was a colored woman æt. 38, of medium height, small in stature, much reduced and emaciated by phthisis pulmonalis; married, one child 13 years old; no miscarriages, and had been in expectancy for some seven months when seen first by me.

According to her own statement, she had been ill of phthisis about two years, the physical signs of which were quite advanced. Through the kindness of Dr. Muncaster, who was attending her at the time for a slight uterine hæmorrhage, I was asked to take charge of her case. A second hæmorrhage occurred some three weeks later, though of little consequence so far as related to the quantity of blood lost.

My suspicion at once was that I had to deal with a case of placenta prævia, though an examination added little to strengthen or verify such a suspicion. The mouth of the womb was well closed, and quite impossible for me to insinuate my finger sufficiently far, within the cervix, to ascertain a placental offering without forcing dilatation, the expediency of which I shall have more to say further on. Principally rest, with little medication, was quite sufficient to check the flow, and my attention was then directed to the upbuilding of her general condition. Nutritious foods, good air, bromine and dilute phosphoric acid constituted the principal means employed. These were apparently effecting the results I had desired, and while waiting an opportune time to induce labor, I was called to see her with another hæmorrhage. Though not profuse, it was alarming, owing to the prostration it occasioned. For some time after my arrival, the hæmorrhage having been checked, she was unable to converse, and only slowly rallied under careful administration of stimulants and artificial heat. At this time there was no longer a question as to the condition, but a grave one, as to the most expedient procedure. To attempt a speedy delivery would be to employ means positively dangerous, and possibly to destroy the little chance that remained for her recovery. I packed the vagina, having previously inserted within the cervical canal a pledget of cotton, and gave my attention to the effort to restore her from the effect of the shock—the result of which was so transitory and unsatisfactory, that it was evident neither a conservative nor radical course of procedure was without danger.

Dr. Reyburn was now asked to see her with me, and found her substantially as above described. I attempted forcible dilatation with the fingers, but found her strength insufficient to bear it, and again we resorted to the tampon and waited dilatation, watching her all the time, and employing such

measures as the case and circumstances would allow. After six hours of watching and waiting, the cervix having in the meantime dilated, we administered chloroform and turned. The feet were brought down, and in ordinary cases, we would have waited for uterine pain and contractions, but in hers, there having been an absolute absence of both from the first, and ergot was without effect in producing them, we considered it hazardous to longer wait, hence proceeded to deliver by accoucheur force. The child being large in proportion to the woman's pelvis, in the absence of maternal aid we found it difficult and slow to relieve her; however, after some time and effort we succeeded in delivering her of a still-born child. The afterbirth soon followed. Uterine contraction was prompt and firm, and no hæmorrhage followed.

Mrs. S. rallied sufficiently to recognize and speak with her husband, nurse, and a friend who stood by her bedside, but shortly began to sink, and thus continued an uninterrupted failure for about three-quarters of an hour, when she expired. All means at hand, such as hypodermics of whisky, atropine, strychnia and aq. ammonia, together with artificial heat, were of no avail.

It may occur to some of you that it might have been wisest, as well as in keeping with a great deal of teaching upon this subject, for me to have induced labor when first I saw her with second hæmorrhage. There are conditions, individual and circumstantial, which should modify our plans and methods of treatment, irrespective, I think, of the fact that some writers have said, in every case of pregnancy when hæmorrhage occurs after the sixth month, premature labor should be induced. While, doubtless, this may serve the end well in many cases, I am strongly inclined to look upon this latter statement as dangerous teaching. Since it is impossible for one to formulate a rule, or set of principles, which will cover all conceivable conditions, it seems both unwise and unwarrantable to ask one to follow absolutely teachings which admit of no qualification. It would seem to conserve the proper end of treatment in any case to use at all times, and under all circumstances, the means which offer the greatest security to life, with the least possible danger. This principle I endeavored to apply to the case before you. When called to see her there was much more to be considered than the question of delivery. This was of little moment compared with the probable results of interference. It was evident to me that she was in no condition to stand the shock which induced labor would in her case occasion, hence the reason why I adopted a conservative procedure, when to prolong gestation was to prolong a threatening danger. The necessity for rapid delivery when induced in this case is, I think, apparent to all, and need not be further referred to. I submit the report to you, and will receive kindly any just criticisms you may have to make.

HYDATIDIFORM MOLE.

Read before the Medical Society of the District of Columbia, April 25, 1888.

BY J. H. MUNDELL, M.D.,
OF WASHINGTON, D. C.

I was called about 11 A.M. on the 12th of April, 1888, to see Mrs. L. T., 19 years of age. Upon reaching the patient's house I was told that she had menstruated last about the 15th day of January. About the first of March she began to suffer with nausea and vomiting. After March 16 the nausea and vomiting became more distressing, accompanied by frequent slight hæmorrhages. A physician was called in, who visited her several times in the next two or three weeks. His remedies failed to give her the slightest relief, and not having made a diagnosis in the case satisfactory either to himself, the patient or her family, he was requested to discontinue his visits. I found her suffering with acute labor pains and flooding very profusely. Upon placing my hand upon her abdomen I found the uterus reaching to the umbilicus, quite hard and of symmetrical shape. A vaginal examination revealed the os uteri dilated just about large enough to admit the introduction of an ordinary sized goose-quill.

Her pulse was very feeble and rapid; indeed, she was almost in a state of syncope. Seeing that miscarriage was inevitable, I immediately proceeded to tampon the vagina carefully and thoroughly, which had the effect of arresting the hæmorrhage for a time. I returned again in about an hour and found that the blood had penetrated the tampon, and a slight hæmorrhage still kept up. I then packed tightly the vagina with as much cotton as I could introduce, and confined it with a T bandage. This had the desired effect of stopping the flow. I had before ordered her to be perfectly still, with her head low, and to eat freely of crushed ice. I visited her again at 6 P.M. and found no hæmorrhage. Upon removing the tampon I found in the vagina, and partially protruding from the os, a large mass of hydatidiform bodies.

By introducing my finger into the womb I scooped out all of the mass I could reach, in quantity about a quart (which I here exhibit, except a portion which was lost by adhering to the bed clothes).

The patient then seemed so very weak and the uterus so extremely sensitive and tender, I desisted from making any further exploration, believing, also, that I had secured about all of its contents. After giving ergot as a precautionary measure, and enjoining perfect quiet and recumbency, I left her for the night. The next day I found she had passed a rather sleepless night; her pulse was 120 and very weak, and her temperature 101°. I ordered warm carbolized vaginal injections, and full doses of quinine with

opium, to procure sleep. She was given also milk, beef-tea and milk punch. On the third day her temperature was 99° and pulse about 112. From that time to the present she has steadily improved, and is now able to sit up, although still very pale and anæmic; has a good appetite, and is taking quinine and iron.

Another form of hydatidiform pregnancy is what is called the fleshy mole, also called the true mole in contradistinction from the false mole, which occurs without impregnation.

About twelve years ago Mrs. T., then about 30 years of age, came to consult me at my office. She was looking very pale and anæmic, She complained of great debility, loss of appetite, and inability to make any exertion or attend to her household duties. She further stated that she had not menstruated for five months. I examined her uterus, but could find no appreciable change or enlargement to indicate pregnancy. Her lungs showed no evidence of phthisis. I put her upon the use of iron, quinine and strychnia, with as much nourishing food as she could take. She improved rapidly. After visiting me at my office several times, each time expressing herself as feeling much better and gaining strength, she changed her place of residence and stopped coming. I thus lost sight of her for about five months, when one afternoon I received a summons to come to her immediately, as she was very ill. When I got there she stated that shortly before sending for me she had been seized suddenly with violent pains in her back and lower portion of her abdomen, accompanied with some discharge of blood, which alarmed her very much, but that soon after dispatching her messenger she was relieved, after passing something hard and solid through the vagina, which, upon inspection, proved to be a fleshy mole of the shape but rather larger in size than a hen's egg. She then informed me for the first time that some three years previous, whilst under the care of the late Dr. Benjamin Thompson, of this city, she had passed a similar lump of flesh.

In this instance she had not menstruated for about nine months, four months before she first came to my office, and five from that time to the day when I visited her at her house.

Our much respected President, Dr. Thomas C. Smith, on the 23d of September last reported to this Society two cases of hydatidiform moles similar to mine, and at the same time so thoroughly discussed in his paper all of the literature relating to the causes and formation of these productions, that I deem it unnecessary for me to occupy the time of the Society by saying anything more on this subject.

THE AMBULANCE TENTS AND WAGONS of Captain Tompkins received the diploma of honor at the Brussels Exhibition.

REPAIR OF A RUPTURED CHORION. REPORT OF A CASE.

BY W. L. SCHENCK, M.D.,
OF OSAGE CITY, KAN.

On the first day of June, 1888, I was called to see Mrs. James L., living six miles south of the city. She had been thrown from a heavy farm wagon which, turning over, struck her across the lower portion of the abdomen and hips with such force that she was unable to move her body or lower limbs for several weeks. She was seven months advanced in her sixth pregnancy. Active uterine pains came upon the second day after the injury, the membranes rupturing on the third with a free discharge of liquor amnii. Despite hypodermic injections of sulph. morph. and atropia, rectal enemata of tinct. opii, etc., the os dilated and the pains and discharge continued for three days, the pains steadily growing less forcible and the waters less abundant, when both ceased and there was gradual improvement in the general condition, but any attempt to walk was accompanied by a free movement of the pubic bones at their symphysis and severe pain in the right side and hip, and she maintained an almost immovable decubitus.

On the 4th of August, at the end of her 280 days, I was again summoned to the patient's bedside. The labor progressed rapidly, the membranes rupturing early, and on reaching the house a healthy boy of 9 pounds awaited my arrival. I removed the secundines, which were buttonholed in the vagina, and carefully examined them. The rent in the chorion through which the child had escaped was near its centre. About midway between this and its attachment to the placenta there was a triangular fold in the membrane (which is very prettily preserved in alcohol), that had united by adhesive inflammation. The chorion was separated from the amnion throughout one-fourth its extent, the inner membrane being thickened and presenting on its outer surface—in the vitriform body—an ecchymosed appearance, the spots of blood varying in size from a pin-point to ⅛ inch in diameter, and being covered with a delicate film of membrane.

Though this case is unique, not having found in my reading a similar one, its possibilities are readily appreciated. There was a rent—solution of continuity—in a serous membrane, which was folded together and held by the parts adjoining so as to close the opening; there was consequent irritation and exudation, and a forced rest that permitted adhesive inflammation—a result that might more frequently obtain after such accidents if we could secure perfect immobility of the parts.

The injury to the mother in this case secured a fixed decubitus. The lesson to be learned from it is a more positive insistence upon perfect rest. We know not in what case we may have a fortunate folding and pressure of the membranes which, if

maintained, may be followed by adhesion and a successful termination of the pregnancy. Not only must *absolute* rest be enforced until there has been sufficient time for union by adhesive inflammation, but a reasonable quiet must be maintained until after delivery.

MEDICAL PROGRESS.

REGULATION OF FLUID INGESTA IN CARDIAC FAILURE.—In a paper on this subject DR. J. BARR says:

Some time ago I had a patient who suffered occasionally from irritation of the urinary passages, with excessive excretions at times of uric acid, and at others of earthy phosphates. It was a case where the use of mineral waters might seem to have been indicated, but, as he had a feeble circulation, I cautioned him, before his visits in two successive years to two celebrated mineral water resorts, to be satisfied with the pure air and heathful exercise to be obtained in those elevated regions, without trying their supposed marvellous springs. On both occasions he was induced to consult a leading physician in each place, who understood all about the wonderful virtues of the waters, with the inevitable result that the waters were freely prescribed. On each occasion the patient returned much worse than when he left, and was afterwards quickly cured by a quinine tonic, and increased oxidation by healthful exercise in a place where there was no mineral spring.

I have, at present, under my care in the Northern Hospital, a patient who was admitted some weeks ago suffering from great mitral constriction, free tricuspid, regurgitation with evidence of commencing stenosis, great enlargement and hardness of the liver from chronic conjestion, large ascitic effusion, general venous turgescence, and urgent dyspnœa. On a dry diet the patient has markedly improved; there is no dyspnœa, the venous turgescence has disappeared, the ascites has lessened, and she is now able to move about the ward, and has the prospect of returning home fit for light household duties. This woman has been several times in another institution, where she was kept on a liquid diet, and as she thought that an excessively morbid interest was taken in her case, on each occasion when she considered that she was near her latter end, she fled home to save a post-mortem examination.

Every drop of liquid which is placed in the stomach, with the exception of part of that which passes away in the fæces, must pass through the right side of the heart, and all of this, except that which is exhaled by the lungs, must pass through the left side before it can be excreted. When, therefore, there is any mechanical obstruction to the passage of the blood through the heart, or, when the effective force of the heart is diminished, any increase in the amount of fluid in circulation must severely handicap the central pump. The velocity of the blood depends on the effective force of the cardiac contractions, and on the mass to be moved, and is inversely as the sectional areas. It therefore follows that when you have got a weak heart and a great amount of blood in the vessels, the circulation becomes very languid, and in a given time comparatively little is presented to the excretory organs. By the use of cardiac tonics to improve the force of the heart's beat, and by reducing the supply of fluid, thus lessening the bulk of blood, we lessen the static condition, and increase the velocity of the circulation. The potential is converted into kinetic energy, there is a more rapid interchange of fluids between the blood and tissues, the hydration of the tissues is lesssened, while the oxygen-carrying power of the blood and the oxidation of effete products are augmented, the congestion of all the internal organs is diminished, and their functional activity heightened.

When there is any marked cardiac failure, the quantity of fluid consumed should be restricted to the smallest possible amount, and, in a large number of cases, one pint of liquid, including the fluid portion of the food, will be found sufficient, while if there be any dropsy, the quantity may, in many cases, be reduced still further.—*Provinc. Med. Jour.*, Aug. 1, 1888.

ALKALIES IN ECZEMA. — DR. FREDERICK PEARSE, acting on the principle that acids applied to an acid-secreting surface and alkalies to an alkaline-secreting surface diminish the respective secretions, has treated a large proportion of cases of eczema for many years by alkaline applications. The method may be termed unphysiological, on the ground that the morbid exudation of the eczematous skin has no parallel with the normal secretion of glands, but his answer for the present must be the results. There is not the slightest doubt that an acid applied to an eczematous surface will irritate and increase the exudation, and he is equally satisfied that alkalies, judiciously applied, will have a contrary effect. Whatever views we may have on the pathology of eczema, the great diversity of methods shows that at present we have no very definite *rationale* of treatment. At the same time he does not argue that alkalies have any specific action. No one who has seen much of eczema can doubt that the associated disorder to which these patients are most subject is that connected with their digestive organs. The internal treatment is therefore undoubtedly of paramount importance. The one thing which appears most successfully to meet this indication is some saline aperient, which produces a "weeping" from the mucous surface of the intestines and also hurries on the contents.

This clinical fact suggests that the cause of eczema lies in some imperfect process of digestion, whereby abnormal chemical compounds are absorbed and, circulating in the blood, irritate especially the skin structures. These compounds are unknown. Not only, however, are saline aperients successful in the treatment of eczema, but alkaline combinations are especially so. The most common prescriptions contain these drugs in endless variety.

This treatment is very successful for adult patients, but it is not nearly so useful in the eczemas of young children. In these cases, however, alkaline external applications are as successful as in those of riper years, if not indeed more so. Whether the fault in adult life lies in deficiency, and that of eczematous children in excess, of secretion of acid gastric juice, is difficult to decide; but the internal administration of alkalies combined with saline aperients, in the eczemas of grown-up people, is that on which the greatest general reliance can be placed. In children, however, an exactly opposite line of treatment will often be found successful. It is especially in scrofulous children and in infants that he has found benefit from the internal administration of hydrochloric acid. These little patients have generally some palpable digestive disturbance. He has frequently found unexpected benefit from the internal administration of nitrohydrochloric acid combined with sulphurous acid, at the same time pursuing the external application of alkalies. These latter may be used as solutions of bicarbonate of sodium (5 or 10 grains to the ounce), sometimes combined with small quantities of glycerine, or as weak solutions of liquor potassæ (10 or 30 drops to the ounce). As a general rule, it may be said that the more acute the eczema, or rather the more copious the exudation, the weaker must be the application. It should be made, whenever possible, continuously.

In chronic cases an alkaline ointment may sometimes be used, such as one containing 10 to 30 grains of bicarbonate of soda to the ounce of vaseline.—*The Practitioner*, October, 1888.

Strongylus Paradoxus.—M. J. Chatin recently exhibited before the Paris Academie de Médecine a specimen of this intestinal worm, passed by a butcher, at Oloron, in the department of the Basses-Pyrénées. The patient had been subject for some time to gastro-intestinal disturbances, and had already passed another specimen of the same parasite. *Strongylus paradoxus* is a small round worm; the exhibited specimen measured a little over half an inch. The species was discovered in the lung of a wild boar, by Ebel, at the end of the last century, shortly after Modeer and Bremser discovered in Sweden and and Vienna that this *Strongylus* infected the bronchial tubes of domesticated swine. Rayer found the parasites, in the same anatomical situation, in a pig in the Paris market in 1844; two years later Bellingham discovered the parasite in Ireland. For long, experts have admitted that *Strongylus paradoxus* infests the lungs of swine, causing an affection which was rare till recently. Dr. Cobbold, in 1879, and M. Chatin, in 1881, both showed that swine were becoming more subject to this worm. The former pointed out its danger when a large number of worms collected in the bronchi. In 1850 the wild boars preserved for the Royal hunt, near Berlin, were decimated by this *Strongylus*. M. Chatin notes that no report of the existence of this species in man can be found in medical literature, but the alleged new species, *Strongylus longevaginatus*, found infesting the lungs of a child, aged 6, at Klausenburg, Transylvania, in 1845, is evidently, in M. Chatin's opinion, identical with *Strongylus paradoxus*. Hence, in reality, the latter species has twice been found in man, and on the first occasion it infected the lung and caused death. The worm is very tenacious of life, and its embryo may remain in water, or completely dried up for a year without undergoing any change; yet, if then swallowed by a pig in fodder or drink, will rapidly develop and propagate. M. Chatin believes that in the case of Oloron the parasite was probably swallowed in drinking-water. Since swine are becoming more and more subject to *Strongylus paradoxus*, he thinks that sanitarians must be watchful, lest man should become frequently subject to the ravages of a worm liable, from its favorite habitat, to cause more mischief than can be wrought by *Tænia*, *Oxyuris*, *Lumbricus*, or even the leech-like *Dochmius duodenalis*, which caused anæmia amongst the men employed in the St. Gothard Tunnel.—*Brit. Medical Journal*, August 18, 1888.

Paquelin Cautery in Epididymitis.—According to Dr. Brewer, good results are derived from the use of the Paquelin cautery. He has employed this method in forty-six cases, and in two cases only was it necessary for the patient to remain in bed after the first application, and in one the real cause of the enforced rest was a coexisting cystitis. The details of the procedure, according to Dr. Halsted, who first suggested it, are as follows: The cautery point is heated to a white hot temperature, and the surface of the skin overlying the affected organ is then lightly touched with the cautery. The operation requires only a few seconds, and need not be more than moderately painful. A dressing of iodoform ointment is then applied, and the patient is instructed to wear a suspensory bandage. Instant relief from pain is said almost invariably to follow the employment of this treatment, and the patient, as a rule, is able to be up and walk about in comparative comfort. The author adds that he has also found that

marked relief from pain in gonorrhœal rheumatism can be obtained by a similar use of the cautery and iodoform ointment.—*Medical Press*, August 1, 1888.

TRAUMATIC SUBDURAL ABSCESS OF THE BRAIN.—In a paper on this subject, in which some cases are recorded, SIR WILLIAM STOKES draws the following conclusions:

1. That after the primary symptoms of cerebral traumatism have subsided, there is frequently a latent period of varying length, during which there are no distinct brain symptoms connected with abscess formation whatever.

2. That their appearance is as a rule sudden, and if uninterfered with, they run a rapidly fatal course.

3. That the occurrence of pus production resulting from cerebral traumatisms is not incompatible with a perfectly apyrexial condition.

4. That this latter fact will probably aid in differentiating traumatic cerebral abscess from meningeal on encephalic inflammation.

5. That both as regards color and consistence there is great variety in the contents of cerebral abscess cavities, and that as shown in Wilm's case, published by Rose, of Berlin, they may become transparent.

6. That antisepticism has largely diminished the risks of the operation of trephining.

7. That having regard to the great mortality of cases of cerebral abscess when uninterfered with, viz., from 90 to 100 per cent., that the operation is indicated even when the patient is in extremis.

8. That in the case where the trephine opening does not correspond to the situation of the abscess, exploratory puncture and aspiration may be employed.

9. That by the adoption of this measure the necessity for multiple trephine openings can be largely obviated.

10. That the employment of a blunt-pointed aspirating needle, as suggested by Rentz, is probably the safest mode of exploration and evacuation.

11. That drainage is desirable in the after treatment of such cases.

12. That both during and subsequent to operative interference in these cases a rigid antisepticism is imperatively required.—*Annals of Surgery*, Oct., 1888.

TREATMENT OF TYPHLITIS.—BOUCHARD'S method is as follows: Soothe the pain by a hypodermatic of morphia, if very sharp; if not use a thick layer of Neapolitan ointment with belladonna, covered by a large and very hot poultice. Give aseptic rectal injections twice a day, with at least a litre of water, to which is added 5 grams of borate of soda, and two or three teaspoonfuls of tincture of benzoin and camphorated alcohol mixed. Give injections very slowly. Patient must have absolute rest. Give only the mildest purgatives, if any—such as magnesia in water. For diet, milk and alkaline water; later, milk with yelks of eggs. If there is at the end of a fortnight, a thickening around the cæcum, apply a small blister.—*Revue Therapeutique*, No. 4, 1888.

SUBSTITUTE FOR NITRITE OF AMYL.—MM. BALS and BROGLIO describe as the nitrous ether of dimethyl-ethyl-carbinol (amylene hydrate) a very mobile liquid, lighter than water, of an amber color, having an odor similar to that of camphor and terpine, insoluble in water, slightly soluble in glycerine, and very soluble in ether, alcohol and chloroform. While its physiological action is more marked than that of amyl nitrite, it does not cause flushing of the face, and as much as 80 or 100 drops can be inhaled daily.

ANTISEPTIC INJECTION IN SEPTICÆMIA.—MOIZARD, in a case of septicæmia occurring in pyo-pneumo-thorax, injected into the pleural sac an ounce of the following mixture:

℞ Tincture of iodine.
Alcohol.
Solution iodide of potass. (1:10) āā ℥ij.

The patient recovered, though the case was a very grave one.—*Union Médicale*, Aug. 11, 1888.

HYPODERMATIC INJECTIONS OF ANTIPYRIN.—DERLON advises the use of cherry-laurel water to obviate the pain of subcutaneous injections of antipyrin. He uses the following formula:

℞. Antipyrin. . . . . grs. 30.
Cocaine hydrochlor. . . . . gr. 1.
Cherry-laurel water. . . . . ℥j.

This should not be used with morphine injections, since chery-laurel water increases the pain in these cases.

COMBINED CHLOROFORM AND COCAINE ANÆSTHESIA.—OBOLISOVSKI proposes to inject cocaine at the site of operation after the patient is chloroformed. He claims the following advantages: 1. The local anæsthesia is more effective. 2. Vomiting occurs less frequently. 3. The waking is easier and the after-weakness less. Nervous subjects are sometimes much excited by the cocaine injection.—*Journal de Médicine de Paris*, Aug. 12, 1888.

SOWTHISTLE; SONCHUS OLERACEUS.—DR. S. F. LANDRY has investigated the hydragogue properties of this plant. It acts strongly on the liver, duodenum and colon, and resembles elaterium in causing large watery discharges. It has given good results in ascites and hydrothorax. It should be combined with carbonate of magnesium to prevent griping, or else with manna, aniseed, or some other aromatic stimulant.

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SATURDAY, OCTOBER 27, 1888.

BACTERIOLOGICAL EXAMINATIONS AT AUTOPSIES.

In reading recent medical literature, says BABES, it seems that a precise bacteriological examination is reserved for special cases of infectious diseases. He is convinced that bacteriological researches should be made systematically in all autopsies in infectious diseases. To this end, in the necropsy on man all the precautions should be taken as in the case of an animal; it should be made in a laboratory, where the micro-organisms in the tissues and organs may be collected. In the *Archives Roumaines de Médecine et de Chirurgie*, Babes has a contribution on the technique of such autopsies.

In the first, he says, scrupulous cleanliness of the hands should be observed. The cadaver may be preserved at a temperature of 5° or 6° C. The body should be washed with a 1:1000 solution of corrosive sublimate, and all the water used during the autopsy should be such a solution. The instruments should be sterilized in a stove. The first section of the skin, from the chin to the pubes, should include the subcutaneous cellular tissue and the muscles, and is made with one sterilized knife. Another sterilized knife is then used to open the peritoneal and thoracic cavities. The liquids contained in the peritoneal, pleural and pericardial cavities are then sterilized by plunging a platinum wire into them, and they are then collected in culture tubes or on plates previously prepared. When these liquids are used for inoculating animals, a sterilized syringe should be used. For the bacteriological examination of organs, the surface of the organ is burned with a hot glass rod over an extent of about 2 cm. The tissue underlying the burn is then torn out. From this fragment is taken, by means of a red-hot platinum wire, a piece to be used for inoculation. The liquids contained in the intestines, bronchi, bladder, ureter, etc., are collected on gelatin plates. At the same time slides are prepared for micro-bacteriological examination by placing on them a few drops taken from the liquids, and leaving them to dry. By means of a sharp sterilized scalpel pieces are removed from the organs and placed in alcohol, for subsequent microscopic examination. At each autopsy at least a dozen slides should be prepared of the liquids and organs, and cultures should be made in moist chambers, and culture tubes with gelatin, agar-agar or serum. During the autopsy several guinea-pigs, rabbits and mice should be inoculated.

We do not know, says Babes, why in man the course of infectious diseases is so variable in different individuals. In the lower animals an experimentally produced infectious disease has, under the same conditions, the same type—an absolute identity, in animals of the same species. "Under the same conditions" seems a sufficient explanation, or at least the key, to the variable course of these diseases in man, since it is very seldom that we find human beings "under the same conditions." Babes says that in making autopsies under the best conditions for the collection of bacteria, he has found that this variability of symptoms in the same infectious disease in different patients is seen especially in cases of complication of the principal disease by secondary infections, also caused by bacteria. In connection with this the reader will remember the editorial article in THE JOURNAL of September 29, on Dr. Bayard Holmes paper on "Secondary Infection in Typhoid Fever." Two years ago Babes presented to the Medical Society of Budapest a bacteriological analysis of autopsies made at the Children's Hospital; and from this it seems that it is the bacteria of diseases consecutive to wounds that more often complicate the infectious diseases and cause death. In the bodies of children dead of infectious diseases, examined a short time after

death, streptococci and some of the five species of saprogenic bacteria are usually found.

An analysis given by Babes shows that it is well to search for and to cultivate the bacteria in all autopsies. Sometimes new bacteria are found, pathogenic for animals. Since in many of the internal diseases the complications that cause death are caused by the same microbes as are found in traumatisms, it may be concluded that at some particular time, by reason of the primary disease, the tissues have lost their power of resistance to the invasion of certain bacteria. This is seen in that curious and exceptional complication, malignant œdema in the course of typhoid fever, as described by Brieger and Ehrlich. We must also take into account the frequency of complications due to the habitual association of certain bacteria with certain determined diseases. As an example of this may be mentioned cutaneous gangrene in malignant pustule, due to the presence of micrococci that constantly accompany the bacteria of charbon. In most cases, as already said, it is the bacteria of pus that penetrate the organs, and cause a septicæmia analogous to that caused by suppurating wounds. The chronic infectious diseases, such as phthisis and leprosy, often show local lesions caused by bacteria, as the streptococcus pyogenes, the diplococcus of pneumonia, and sometimes the tetragenus, in tuberculosis; these are habitual causes of the various complications on the part of the lungs or other organs. In leprosy staphylococci are often found. In actinomycosis the abscesses are often found to contain the staphylococcus aureus and streptococcus pyogenes, and it is very probable that the production of pus is due especially to the bacteria that enter secondarily, after the organisms that cause the primary disease.

There are also a number of diseases not caused by bacteria (or not known to be thus caused) that may become the seat of numbers of micro-oganisms. This is true of certain tumors, which ulcerate and are invaded by colonies of bacteria. As a rule, tumors are less resistant to the invasion of bacteria than sound tissues. The paper of Babes is a very suggestive one, and his suggestions well worth careful consideration.

DR. JOS. M. WOOD, of Kansas City, died on September 19. He was born in 1810 in Kentucky, and began practice in 1830.

SIR DYCE DUCKWORTH ON MEDICAL BULLETINS.

In a recent lecture to the students of St. Bartholomew's Hospital SIR DYCE DUCKWORTH said: I am strongly of the opinion that at the present time a great deal of apprehension and unnecessary suffering is entailed on our patients, especially in the upper classes of society, by the details and clinical minutiæ that too often and most improperly find their place in the bulletins respecting important persons. Such a practice should be firmly discountenanced on every account, and it is certainly satisfactory to know that it does not emanate from the highest ranks of our profession. I believe, further, that many of our patients suffer nowadays more, and are actually less amenable to treatment, than was formerly the case, because there is so much widely spread knowledge of disease conveyed in lay periodicals, and because so many advertisements of vaunted specifics and new remedies are puffed before the public.

Sir Dyce Duckworth said the foregoing in connection with the discussion in clinical lectures, before the patients, of the causation and morbid anatomy, and referred to the practice of the late Dr. Peter Mere Latham, who always withdrew his class from the bedside to discuss these matters. Sir Dyce thinks that such discussions are both inhuman and improper; that we have no right to discuss the horrors of the deadhouse in the presence of patients committed to our charge. While these discussions do not shock us, they may shock and greatly injure the patients, and we must never forget that it is our first duty to be humane, and to regard the patient's interest as first and paramount.

In regard to medical bulletins it may be said that they are the outgrowth of recent years, especially due to the so-called "enterprise" of the newspapers, which first persuaded themselves, and then the physicians, that the public must have all the details possible. In the case of distinguished persons there can be but little objection to the physicians letting the public know how they are in general terms; but it certainly is no business of the public to know all the minutiæ of symptoms and treatment, how often a laryngeal mirror is used, a catheter passed, or a nutrient enema given. The newspaper writers and the public do not understand more than a small fraction of the medical terms used in minute bulletins.

and can get but an erroneous impression from them. The fierce quarrel now in progress between Sir Morell Mackenzie and the German physicians in the case of the late Emperor Frederick should be a lasting warning to physicians not to issue minute bulletins, nor air the progress of cases in the newspapers. In the case of a public man ill, the public is interested in the case only to the extent of knowing in a general way *how* he is, not why he is better or worse, what his daily symptoms are, and what the plan of treatment is.

As we all know, the physicians in a case cannot always agree; when they do not agree, and issue bulletins, these are sure to bring the whole matter of disagreement before the public. As a result we find in the newspapers such editorial comments as the following:

"The German bureaucrats are busy burning the book of Dr. Mackenzie. The work shows that it was not he but the native doctors who hastened Frederick to his grave. The physicians of Fatherland will reply with a hundred books ar least, and the question of our youth, "Who Killed Cock-Robin?" will be forgotten in the quest for the great healer who, for a fee of one hundred thousand, gave the sick Emperor that particular *coup* which served him.

"Yet Frederick is as dead as his ancestor, the great Elector. What do the doctors gain by revealing the foibles and the impotency of their profession? So long as no respectable man will die without the presence and attention of a doctor, why should not the learned men rest contented with the respect in which they are held?"

What do we gain but a certain amount of distrust on the part of the public, and too frequently a snub when we go before the public with a matter of importance to the public?

EDITORIAL NOTES.

Hysterical Spasm of the Œsophagus.—Borgiotti records an interesting case of a woman, aged 31, who, having had four attacks of hysterical convulsions, was taken with spasmodic dysphagia at the end of a fever that lasted two months; in addition to the dysphagia was pyrexia, frontal headache, severe pains behind the sternum. At each attempt to swallow, all food was rejected. This œsophageal spasm continued without interruption for 530 days. Occasionally it could be overcome by means of a sound, but it rarely permitted the passage of even fluid food. The part of the œsophagus affected corresponded with the dorsal spine from the fourth to the seventh cervical vertebra; at this point the passage of the sound caused pain. The spasm was finally cured by means of Verneuil's œsophageal dilator. After the first dilatation fluid food could be introduced by means of the sound. On the sixth day the patient could swallow solid food. During the first six days the patient's weight increased five pounds, and the temperature, which had been subnormal for some time, rose to normal.

Prevention of Ophthalmia.—Dr. Littlejohn, Medical Officer to the Hanwell School, London, at the conclusion of a report on ophthalmia in the central school district of London, summarizes his suggestions as follows: 1. The division of those attacked into small groups. 2. The appointment of a sufficient number of experienced nurses. 3. Abundant open-air recreation for those attacked. 4. A system of cleaning school-rooms, etc., which shall avoid damp boards as the result. Abundant ventilation, especially of dormitories; artificial warmth and extra clothing being utilized when needed. 6. Improvements in bathing and washing; including the provision of large open-air baths. 7. The washing and efficient disinfection of each towel after use. 8. More outing in the country lanes during half-holidays; together with provision of means for exercise and games. 9. Such arrangement of seats in the school-rooms that there shall be no strain on the eyes, and that the sun's rays shall not fall directly on the eyes.

Electrolysis in Parenchymatous Goitre.—Weinbaum, of Kovel, describes in a recent number of *Vratch* two cases of parenchymatous goitre cured by electrolysis. The electricity was applied for ten or fifteen minutes at a sitting, supplied by a battery of 20 cells connected with two gold needles; these were thrust several millimetres into the tumor at two points diametrically opposite. Moderately strong currents only were used. In the first case one hundred and fifty séances were had in eight months, during which time the tumor gradually dwindled away. The patient was seen about a year afterwards, and was in complete health. There was no trace of

the swelling except slight cicatrices at the sites of small ulcers made by the needles. In the second case there was but little left of the goitre after fifty séances, and the general health was much improved. Weinbaum tried electrolysis, but unsuccessfully, in a case of dense fibrous goitre.

YELLOW FEVER continues at Jacksonville, Florida, though the number of cases are greatly diminished. The number of new cases reported on the 22d inst. is 43, and deaths, 1. Of the new cases 8 were whites and 35 colored. The whole number of cases reported since the commencement of the epidemic in Jacksonville is 3,839, and the number of deaths to date, Oct. 22, 332. The danger of any general spread of the disease through the Southern States the present season has evidently passed, and most of the quarantine arrangements outside of Florida have been discontinued. The volunteer physicians who have been on duty at Jacksonville are reported to have tendered their resignations because their services are no longer necessary.

APPARATUS FOR RECORDING LATERAL CURVATURES OF THE SPINE. — DR. C. L. SCUDDER describes in the *Boston Medical and Surgical Journal* an apparatus that records:

1. The lateral curvature of the spinous processes of the vertebræ. 2. The outline, height, and breadth of the shoulders. 3. The outline, height, and breadth of the hips. 4. The relation between the arms in the natural position and the outline of the hips. 5. The distance of the scapulæ from the spine. 6. The antero-posterior curves of the back. 7. The curve of rotation in the dorsal region. 8. The curve of rotation in the lumbar region. The tracings may be easily reduced for records.

AN ITALIAN MEDICAL SOCIETY, on the plan of the German Society of Internal Medicine, is proposed under the name Società Italiana di Medicina Interna. The proposition was to hold the first meeting in Rome this month. Discussions on any one subject are not to exceed one sitting, the introducers of the discussion being allowed half an hour, and subsequent speakers a quarter of an hour. Each introducer is to have ten minutes to close the discussion. Papers must be read within twenty minutes, and for the discussion of them each speaker is allowed ten minutes, no person being allowed to speak more than once.

OUABAÏNE, $C_{30}H_{46}O_{12}$, is a crystallizable derivative of Ouabaïo, which belongs to the family Apocynaceæ and grows in the mountains of Comal, Eastern Africa. ARNAÚD has isolated from the wood about 30 per cent. (!) of the substance he calls ouabaïne, which is slightly soluble in cold water, and very soluble in boiling water, and insoluble in chloroform, anhydrous ether and absolute alcohol. The physiological experiments made thus far show, according to Varigny and Langlois, and E. Gley, that its physioiogical action is similar to that of strophanthus.

A DECISION IN REGARD TO MORAL INSANITY has been given by the Supreme Court of Connecticut as follows (in Anderson *vs.* The State, 43 Conn., 515): It is true that courts have been slow to recognize this form of insanity as an excuse for crime. Nevertheless, that it exists is well understood, and that in some cases it is clearly defined.

THE VACCINE AND EXPERIMENTAL LABORATORY of the University of Missouri, at Columbia, now announces through Dr. P. Paquine, State Veterinarian, that bovine vaccine will be supplied in the State of Missouri in the first week in November, and the first weeks of February, May and August, 1889.

THE JOURNAL OF OPHTHALMOLOGY, OTOLOGY AND LARYNGOLGY, is the title of a new periodical announced to be commenced in January, 1889, in New York, and edited by Geo. S. Norton, M.D., assisted by Charles Deady, M.D. It is to be issued quarterly, at $3.00 per annum.

THE SALINE COUNTY (KANSAS) MEDICAL SOCIETY was organized on August 28, with Dr. A. S. M. Anderson, of Salina, President, and Dr. J. R. Crawford, of Assaria, Secretary, and with a charter membership of ten. It meets on the last Tuesday of each month.

DR. GAMALEIA has gone to Paris to explain his system of vaccination against Asiatic cholera. He proposes to go to India in March, to test his methods on cholera patients.

THE SOUTH KANSAS MEDICAL SOCIETY will hold its semi-annual meeting at Hutchinson, Kansas, on November 13.

SOCIETY PROCEEDINGS.

Medical Society of the District of Columbia.

Stated Meeting April 25, 1888.

THE PRESIDENT, T. C. SMITH, M.D., IN THE CHAIR.

DR. J. H. MUNDELL read the history of the case and presented the specimen of

AN HYDATIDIFORM MOLE.

(See p. 592.)

DR. SMITH stated that this was the fifth growth of this character presented to the Society within the past year, thus apparently militating against the statement that it is rare.

DR. PHILIP MARVEL read the history of a

CASE OF PLACENTA PRÆVIA.

(See p. 591.)

Dr. MARVEL, in opening his paper, stated that he only wished to confine his remarks to the report of a single case. He had seen other cases, but did not have the requisite data. He interpolated that ergot and pulsatilla failed to produce contraction of the uterus, and that he had never known pulsatilla fail before.

DR. REYBURN had assisted Dr. Marvel in the case. He had regarded the case as hopeless when he saw the woman on the day of the delivery, on account of pulmonary phthisis and a narrow pelvis. She was almost exsanguinated and was without uterine pains—they being absolutely absent notwithstanding the administration of ergot, pulsatilla, stimulants, strychnia, liq. ammoniæ, etc. In reviewing the case he could not see anything that could have been done that was left undone.

THE PRESIDENT asked Dr. A. F. A. King to give the Society the recent orthodox treatment of placenta prævia.

DR. KING: The present orthodox treatment is gradual version by the bipolar method, and getting down one foot and letting it act as a plug, and then bringing down the child as gradually as the cervical relaxation will permit. It is very easy to criticise anyone's treatment of a case. No one man's practice could be taken as a criterion, on account of the small number of cases. But he strongly advised the German bipolar gradual version. He thought it was in just such a case as this that Simpson would have removed the whole placenta first, thus preventing hæmorrhage.

DR. FRIEDRICH had had a similar case on the 10th inst. The woman was in the water-closet and thought she was having a stool, but she was flooding. He tamponed and stopped the hæmorrhage. In the morning he removed the plug and the hæmorrhage began again. Dr. W. W. Johnston was called in consultation and advised him to continue the treatment and wait for pains. Dr. J. returned at 10 P.M. and, there being some dilatation, they separated the placenta, turned and delivered, but the woman died about an hour later.

DR. KING: What did Dr. Friedrich mean by separating the placenta:

DR. FRIEDRICH: Partial separation.

DR. KING: Rupture of the membranes in the beginning without regard to the viability of the child was recommended.

DR. OBER had seen a lady about five months ago for another physician. Digital examination revealed a complete placenta prævia. The hæmorrhage had been very insignificant and there was absolutely no uterine contraction. He thought best to temporize. When the attending physician returned, about forty-eight hours later, he thought best to deliver at once, notwithstanding the flabby condition of the uterus, to which his attention was called. The doctor turned and delivered at once without an anæsthetic. Four hours after delivery and two hours after Dr. Ober had left the house, the woman died of post-partum hæmorrhage, though she was in excellent condition at the time of delivery. The child lived. In Dr. Friedrich's case the woman did not seem to be much affected by the anæsthetic, though hard to get under its influence.

DR. REYBURN, about three years ago, had seen a case with Dr. Morgan in which both mother and child lived. The head of the child descended and labor progressed well after partial separation of the placenta.

DR. SMITH thought it very unwise to trust to the head as a tampon acting against the margin of the uterus. He succeeded in delivering the child alive in a case in which he had previously used forceps, but the woman died. The next case he lost was one in which he had advised extraction of the child after a slight hæmorrhage upon walking about the floor. About two weeks later she flooded and was delivered by nature, and died of exhaustion about a week later. This case was seen with Dr. Burrows, who told him that he had seen "six cases of placenta prævia, five of which died and the other was not a case of placenta prævia."

DR. M. MUNCASTER favored the use of cannabis indica in 10 to 15 drop doses.

DR. MARVEL had seen four cases of placenta prævia: one in his own practice, two in consultation, and the one reported to-night. In his own case he thought it was hydrorrhœa at the first flow, and it was not until the second hæmorrhage, at the eighth month, when he made out partial placental detachment. In one case both mother and child lived. In three the mothers died. In two he used Barnes' dilators; in one turned. Would like to have heard something about the causes of placenta prævia. According to Goodwin, Mundé, Lusk and others, rapid labor should

be induced after the sixth or seventh month. Penrose taught to temporize at first, but if danger threatened to dilate and deliver. Goodell sanctions this.

Stated Meeting, May 16, 1888.

THE PRESIDENT IN THE CHAIR.

DR. ROBERT T. EDES read a report of

A CASE OF CEREBRAL TUMOR.

History.—A lady, aged 58, some three years ago had neuralgia in the head, which was cured, as she supposed, by massage. For two years she had epileptiform convulsions, together with slighter attacks resembling petit mal. Both were followed by temporary aphasia, and the more severe ones by slight signs of right hemiparesis. Headaches were neither frequent, severe nor localized. There was no hemianopsia, and the fundus of the eye, which, however, was not examined for nearly a year before death, was normal. A few weeks before death the aphasia and paretic symptoms became more marked and persistent.

There was a sarcoma on the left occipito-temporal lobe between the fourth convolution (lobulus fusiformis) and the posterior horn of the ventricle. The point of the temporal bone was softened. The island of Reil was swollen and pale, and under the microscope showed proliferation of the superficial layer and degeneration of the deeper pyramidal cells.

DR. SCHAEFFER had examined a tumor of this kind removed from a patient of Dr. J. T. Young. It was a sarcoma, situated in the left anterior lobe, and surrounded by a distinct capsule. The history was very meagre; headache was very prominent and was the only symptom dwelt upon by the physician. Mental derangement was absent. The tumor was adherent to the inner surface of the temporal bone, ovoid in shape, and one and a half to two inches long.

DR. KLEINSCHMIDT: The paper gives a correct history of a very interesting case. The diagnosis was in doubt and could only be made with difficulty. The recent advances in brain surgery make this case more important. Could it have been possible to have made out the location of the tumor in this case from the history? By no means. A lesion may involve either the frontal, occipital or sphenoidal lobe without showing any motor, sensory or special symptoms. This tumor was located in a locality of which we know the least. There was no disturbance of vision. Dr. Edes said there was amnesic aphasia and correctly believes that the lesion in the island of Reil was not the most important.

DR. EDES: The symptoms were due to the secondary lesion and not to the primary. It was not possible to have diagnosticated the location of the tumor from the symptoms.

(*To be concluded.*)

Gynæcological Society of Chicago.

Regular Meeting, Friday, June 29, 1888.

THE PRESIDENT, HENRY T. BYFORD, M.D., IN THE CHAIR.

DR. DANIEL T. NELSON presented a

SARCOMA OF THE OVARY WITH HALF-TWISTED PEDICLE, REMOVED BY AUTOPSY.

I first saw the patient in consultation several days before she was taken to the hospital. She was a patient of Dr. J. E. DeWolf, of Englewood, whom I invited to be present to-night, but unfortunately, he had a professional engagement. She was taken to the Woman's Hospital, but operation was delayed from one day to another, waiting for her to improve in condition, which she never did, and we have the tumor here by post-mortem removal. Her history is very scant, and yet some points in it are of interest, and will raise queries that I trust some of you will be able to answer.

Mrs. M. entered the Woman's Hospital June 7, 1888; occupation, housewife; age at puberty, 12; age on entering the hospital, 39. She was born in America of French and German parents; twice married; the first time seven months, second time seventeen years. She was the mother of nine children, one by her first husband and eight by her second. After the birth of her last child, seven years ago, she did not menstruate for four years; since that time there have been irregular menstrual periods. It is so stated in the history, and yet I think we should rather say there were hæmorrhages from the uterus during these past four years. One year ago she noticed a fluid discharge from the rectum. This is a nice question in pathology, to my mind. She gave evidence of some inflammatory process in the right ovarian region—tenderness, soreness, some elevation of temperature, was confined to bed for a time, and there was a sudden discharge of a considerable quantity of blood. Such quantities are never rightly estimated, but the amount was guessed at by the patient at more than a pint, and supposed by her to have passed from the rectum. Perhaps that was not correct; at all events, after that bloody discharge, she was relieved of the swelling, the tenderness, the inflammatory process, whatever it was, and resumed her ordinary duties. Some time afterward, but unfortunately the record does not say how soon afterward, she began to suffer from swelling in the same region, that continued up to her death. There was constant soreness in the right inguinal region; three months ago the abdomen began to enlarge and she to gain in flesh, strength, and vigor, so that her attending physician, without making a local examination, and especially her neighbors, supposed she was pregnant. She felt

comparatively well until four weeks previous to entering the hospital, when she began to suffer severe pain, tenderness in the right inguinal region, and there was evidence of some kind of tumor. On going to bed with her last illness, about a week before she entered the hospital, her physician became satisfied that there was something more than pregnancy, that there was inflammation of some type. Some days before she entered the hospital I saw her in consultation, and advised a removal to the hospital in the hope that there might be some kind of an operation for her relief. On entering, her temperature was 100⅖° F., pulse 104. The following day the temperature was 100⅘° F., pulse 132; the following afternoon the pulse was 132, temperature 100° F. and a fraction. On the morning of the fourth day the temperature ran down to 99½° F., and the pulse to 119. Possibly an operation might have been performed then, and her life saved, but a more convenient and better time was sought for, that never came. There were the usual evidences of peritonitis, and death in the usual way followed. When she first entered the hospital her bowels were moved, but not afterward; vomiting came on the third day, but passed off on the fourth, when probably an operation could have been performed with the possibility of saving life. She died on the sixth day after entering the hospital, and a few hours after death a post-mortem examination was made and this tumor removed. The appearance is somewhat changed now, but yet it presents fairly well the appearance at the time of the autopsy. You notice the dark, venous, congested appearance of a portion of the tumor. This was the anterior portion as it presented against the abdominal wall, very slightly adherent; no adhesions from old inflammation, either to omentum or other structures, but *a half-twisted pedicle*. The pedicle has been tied in such a way as to retain that appearance as much as possible. Here we have the broad ligament that is simply half-twisted and tied in that position on purpose. The evidence of completely twisted pedicle and death of the tumor were not present. There was simply an increase in size resulting from the congestion, but no sloughing, no death of the part—a slow, inflammatory process had taken place in the tumor and subsequently in the peritoneum, that was the cause of death. The obstruction of the bowels, I believe, was due to the peritonitis, and not to pressure from the tumor. It has not been my privilege to see a patient with a tumor and twisted pedicle, but it seems to me I could have recognized it; but this being only half-twisted, the circulation was impeded, not stopped. The tumor has been examined by Dr. Frank Carey, and the report is sarcoma. There was, so far as I saw, and I made a rather hurried examination, no evidence of the disease extending to other organs; there is no evidence of it in the pedicle; there was no evidence in the glands or intestines or structures adjacent, so it seems as if it could have been entirely removed if the operation had been performed during the life of the patient. The uterus was a little enlarged, but no other evidence of disease about it. I made a diagnosis of malignant tumor, without being exactly certain as to its nature, but it seemed to me malignant on account of its rapid development and the age of the patient. I did not regard it as a uterine tumor, as the uterus was movable and the tumor seemed to be separate from it. Within the abdominal walls there was a considerable amount of ascitic fluid, so that the abdomen was very tense, and it was difficult to say whether or no the tumor could be moved readily within the abdominal walls. I was unable to say whether or no there were adhesions, but from the ascites I hoped not.

DR. ETHERIDGE: The doctor said that if the pedicle had been completely twisted he could have determined it. I would like to ask how?

DR. NELSON: The evidences of acute inflammation would have been much more rapid and severe, also the appearance of shock. In other words, the patient would be something in the condition of one with an internal hæmorrhage, there would be evidence of greater disturbance that would come on suddenly after exercise, while in this case there was no sudden beginning of the evidence of inflammation, it came on gradually.

DR. ETHERIDGE: Did you diagnose a solid tumor?

DR. NELSON: It seemed to me that it was; there was considerable fluctuation and ascitic fluid, but it seemed to me a solid tumor, and that was my reason, together with the age of the patient, for believing it was malignant. I supposed it was carcinoma and not sarcoma. I would like to ask whether that hæmorrhage, indefinitely described as from the rectum, could by any possibility have been the result of a congested condition of the tumor that was freed by an opening through the Fallopian tube, the pedicle having been untwisted, and whether the attack a year or more ago was similar to the one she died of, only the pedicle was more twisted this time, so nature could not relieve herself in this way. In reply to Dr. Etheridge, he said that the rectum was not examined.

DR. FENGER: As a rule, under other circumstances, malignant tumors have as one of their main characteristics invasion of the surrounding tissues and, consequently, adhesion and inflammation enough to bind the tumor to the surrounding organs. But we know that, in sarcomas or carcinomas of the ovary, it is common to find, as in this case, no adhesions. This fact is probably explained by the early setting in of ascites, as we know that the presence of fluid, ascitic fluid, saline solution, etc., in the abdominal cavity, helps

to prevent adhesive inflammation by keeping the tumor away from the loops of intestine.

DR. HENRY T. BYFORD: I examined the patient once, in life, and was present at the *post-mortem* examination. I satisfied myself that the tumor was not connected with the uterus, for although pressing upon the tumor moved the uterus, yet lifting the tumor did not. The course of the disease appears to have been, first, the twisting of the pedicle, then venous congestion, bursting of small blood-vessels, rapid distension and inflammation of the tumor, especially on the side that we now see to be black. The case was not one of ordinary peritonitis; there was not much tenderness, except when the tumor was directly pressed upon. Intermittent attacks of partial obstruction of the bowels, due, undoubtedly, to the presence of this heavy tumor, hastened her death.

DR. CHRISTIAN FENGER presented a

FIBRO-CYSTO-SARCOMA OF THE UTERUS.

This specimen was removed by laparotomy from a woman of 35, who had a tumor the size of a child's head, immovably connected with the uterus at the fundus, and also two small myomas that could be felt through the vagina. The large tumor showed fluctuating places on the surface, by palpation through the abdominal wall, and I concluded that it was an ovarian cystoma, either located in the broad ligament or sufficiently adherent to the uterus to make them move together. At the operation I found it to be a cysto fibroma, or fibro-cysto-sarcoma, subperitoneal, but attached by the broad base to the uterus at the fundus. After temporary elastic constriction around the cervix, the tumors were enucleated, and as the uterine cavity was not opened, I united the wound of the wall of the uterus with buried step sutures, deep and superficial, and a final continuous suture along the inverted borders of the peritoneum.

At the close of the operation all hæmorrhage had apparently stopped, consequently I did not drain. In the course of the first week some fever set in, and on the tenth day I reopened the lower border of the wound and evacuated about three to four ounces of blood mixed with pus, from a cavity surrounding the body of the uterus. The evacuation and subsequent washing out and drainage did not have much influence on the patient's condition; the fever continued, she had a large gangrenous bed-sore over the os sacrum and died six days later, in the third week after the operation. The autopsy showed no peritonitis, and the cavity with the accumulation of blood and pus was found entirely separate from the general peritoneal cavity. On examining the uterus I found, as you see here, surrounding the line of the uterine wound, an island of gangrenous tissue including the wound and a square inch or more to each side. This gangrene explains the persistence of fever and sepsis, notwithstanding the evacuation and drainage.

The large inner tumor has, you see, a smooth surface. On the cut surface, in some parts, there was an appearance of myoma, in other places, islands of softer tissue looking like myxoma or sarcoma, and in other parts cystic cavities. These cysts have not the usual shape and appearance of cystomata, but are irregular, triangular, or longitudinal sinuses, the walls of which are not smooth but trabeculated, so as to give the appearance, as Dupuytren describes it, "similar to the walls of the ventricles of the heart."

I shall here make a few remarks on fibrocystomata of the uterus, because they are comparatively rare, the whole number described in the literature not being much above 100. Fibrocystomata are, as the name indicates, forms of fibromata or myomata, and it is a comparatively rare change in the preëxisting elements of these tumors that gives them the additional characteristics of cystomata.

We distinguish between the following varieties: myxomyoma, as described by Virchow, characterized by œdema of the interstitial tissue, and by the fluid in the spaces containing mucin; consequently it is something more than a simple œdema of the myoma. Spread islands of embryonal cells are also proof of a more active process, terminating in myxomatous or even sarcomatous tissue. Besides the œdema in the interstitial tissue of the myoma, we find œdema and atrophy of the muscular fibres, isolated fibres or their débris mixed with the fluid in the cavities. These cavities are of all sizes, from the microscopic, as shown on this slide, up to the size of a pin's head or walnut, and we even find cavities of enormous size, containing several quarts of fluid. The cavities are lined with pavement-celled epithelium, or rather endothelium, as you would expect, since they originate from dilated lymph spaces, or naked when the cavity is formed by the disintegration of muscular fibres. The cavities contain clear, colorless, or bloody fluid that often coagulates spontaneously when evacuated—a fact that Atlee pointed out as a differential diagnostic sign in contradistinction to the fluid from ovarian cystomata. A special form is described as fibromyoma lymphangiectodes, by Leopold. Distinctly different from this is the myoma teleangiectodes sive cavernosum of Virchow, with multiple cavities from the size of a millet-seed to that of a pea, communicating with the blood-vessels and consequently containing pure blood. These tumors are found to enlarge during menstruation (Virchow), and on auscultation a bruit is heard (Péan).

As to the place of development, the great majority are subperitoneal. Of the 70 cases gathered from the literature by Heer, 63 were subserous, 5 interstitial and only 2 submucous tumors. They

sometimes attain an enormous size, weighing 29, 40, and in one case even 81 lbs.

The cystofibromata are most often found between the ages of 30 and 50. The symptoms are in the main, of course, the same as those of common myomata and fibromata. Uterine hæmorrhage is rare because, as before mentioned, they rarely develop close to the mucous membrane. A more characteristic symptom is a sudden enlargement, probably from acute increase in the size of the cysts or from intracystic hæmorrhage. The spontaneous coagulation of the fluid would be a valuable symptom if it was constantly found, but in about seventy cases it was noted in only eleven (Heer). It might, however, in reality be more frequent, since in a number of cases it might not have been noticed (Gusserow). The lack of vitality shown by the tendency to local gangrene is also somewhat characteristic of these tumors. Thus Grammaticati, as stated by Gusserow, saw a myoma the size of a child's head, located in the wall of the cervix, undergo superficial necrosis, followed by sepsis and death.

It is rather noteworthy that a correct diagnosis was rarely made. They were almost always taken for ovarian cystomata, and a number of them were punctured. Puncture, however, in this form of cystoma, is far more dangerous than in other cystomata, as shown by Leopold, who found that, as a consequence of puncture, ten patients out of eleven died. McGuire, therefore, is right in asserting that exploratory laparotomy is less dangerous than puncture.

The treatment should be early extirpation, because of the probability of rapid enlargement, the danger of puncture, the liability to gangrenous or septic changes, and thrombosis of the vessels in and around the tumor. Gusserow gives a series of 41 laparotomies with 22 recoveries, the cause of the high mortality being the necessity of the removal of the uterus in some of the cases. Occasionally the operation cannot be finished; thus, according to Gusserow, in 38 cases, 7 were unfinished, and of the 7, 6 patients died. That an exact diagnosis, with a definite premeditated plan of operation, is of extreme importance, is shown by Gusserow, who, out of 11 cases described in the literature, reported 9 recoveries.

A few words about uterine sarcomata, inasmuch as the tumor here presented is a mixed form of cystofibroma and sarcoma. In the uterus we distinguish between circumscribed and diffused sarcomata, the former originating in the muscular wall of the uterus, the latter in the mucous membrane. The circumscribed uterine sarcomata are of the most interest to us in this connection, as they stand in near relationship to fibromyomata and fibrocystomata. They form, usually, round, circumscribed, harder or softer tumors, looking like, and developing in the same places as the fibromyomata, and so similar to these that we must class the relapsing fibromata of Paget among the sarcomata. But besides more or less typical fibrous or muscular cells, here we find islands of short, spindle-shaped, round or polymorphous cells, or islands of myxoma tissue; in general, a more vivid cell-formation than in fibromata and myomata; and we further find in the same tumor in different places different forms of cells. So predominating, however, are fibroma or myoma tissue cells that Schröder regards it as a law that the circumscribed sarcomata are always formed by transformation of fibromata. According to Gusserow, the transformation of fibromata into the mixed form of fibrosarcomata, myxosarcomata, and cystosarcomata is so rare that the literature shows very few well-observed cases of this kind. By examining the microscopic slides that I exhibit to-night we find, in some portions, apparently typical myofibroma tissue, without or with dilated lymph spaces, in which we find granulated matter containing loose or isolated muscular cells; in other places, islands of typical myxoma tissue, here and there islands of embyronal cells; in another part of the tumor, territories of short, spindle-shaped cells, large and with oval or round nuclei; in other words, islands of unmistakable sarcoma tissue; and finally, places of common typical, round-celled sarcoma tissue.

As to the age in which fibrosarcomata of the uterus are found, there is this difference from the cystofibromata, that, while they both are most common between the ages of 30 and 50, the sarcomata are still common between 50 and 60, while the cystofibromata, as we have seen, stop at the age of 50.

As regards treatment, the sarcoma is a malignant tumor and needs more extensive removal or radical treatment than the benignant cystofibroma. The removal of subserous or interstitial fibrosarcomata by abdominal supra-vaginal extirpation and extraperitoneal treatment has often been followed by a growth of sarcomatous tissue in the cicatrix in the abdominal wall. The abdominal total extirpation of the uterus can hardly be said to have lost much of its dreadful mortality of about 70 per cent., from the time of Freund's first operation till now.

In the treatment of this case, the following suggestion occurred to me—a suggestion which was not carried out because of the patient's death. I should operate as I did, enucleating the subserous tumor, and if the uterine cavity was not opened, try intraperitoneal treatment of the stump. After recovery from this operation, if the microscopic examination of the tumor proved it to be a fibrosarcoma, I should follow, as soon as the patient's strength would permit, by vaginal extirpation. In the rare cases in which the size of a diagnosed circumscribed uterine sarcoma or fibrocystoma will permit of vaginal extirpation, this operation is, of course, the only one indicated.

The two other specimens are not strictly gynecological, as they occurred in men. However, they had this in common with gynecology, that laparotomy had to be done.

COLLOID CARCINOMA OF THE CÆCUM.

This specimen is a tumor of the cæcum, a so-called colloid carcinoma. The patient was a man of about forty, in whom, for about six months, an increasing tumor had developed in the middle of the abdominal cavity. When I saw him the tumor was of the size of the head of a child of four, was somewhat movable from side to side and up and down. There were never any disturbances from the side of the intestines, but emaciation and considerable pain. I thought it a tumor of the omentum on account of its mobility, also that it was malignant because it was hard, nodular, and of rapid growth, but I did not think of the intestine being the seat because there were no symptoms. When the abdominal cavity was opened I found this large nodulated tumor with a great many adhesions to the omentum and some to the intestines, and finally having separated these and applied a great many ligatures, when I got the tumor isolated and out through the abdominal wound, I found the ileum passing into one side of the tumor and the ascending colon coming out of the other side. I then divided the ileum and ascending colon two inches away from the tumor, detached and ligated the mesentery, and after the removal of the tumor closed the ileum and ascending colon in the usual way by invagination and suture, and made an anastomosis between the lower end of the ileum and upper end of the ascending colon by means of Senn's decalcified bone plates. The territory of approximation was covered by an undetached omental flap. I preferred this operation to circular resection or implantation of the ileum into the colon, because of the shortness of the plate operation as compared with the others. The patient lived four days, was able to take some liquid nourishment, had no vomiting, no tympanites, showed no symptoms of sepsis or peritonitis, but gradually became weaker and died. The autopsy showed no peritonitis, the ends of the upper and lower bowel were closed, as you see in this specimen, the closed ileum and closed end of the ascending colon, and at a distance of two and a half inches the anastomosis covered with the omental flap, which did not adhere. The peritoneal surfaces between the plates are perfectly united, allowing of no escape of liquid or air. The passage between the ileum and colon is perfectly free, as you see after opening the opposite wall of the intestines. The tumor shows at this point the ileum entering the large irregular cavity containing some liquid fæces, slightly tinged with blood, and at the upper end of the cavity is the ascending color. This enormously thickened wall of the cavity, one and a half to two inches in thickness, is the carcinomatous intestinal wall, the cut surface presenting the characteristic gelatinous appearance of colloid carcinoma. This form of carcinoma has as its characteristics, in distinction from other carcinomas, colloid degeneration of the cells, causing them to enlarge, meet together, and form this transparent gelatinous substance. While we do not recognize a colloid carcinoma as distinctly different from carcinomas in general, as we know that partial colloid degeneration is common in all carcinomas of the intestinal tract; clinically, we recognize the extremes of this degeneration as a distinct form, characterized by its enormous size, and not uncommon in the stomach, large intestine, and peritoneum. In the peritoneal cavity there were no secondary tumors nor were the lymph glands of the mesentery invaded. This is what we should expect, as this colloid carcinoma is, as a rule, relatively benignant, with little tendency to the invasion of distant tissues or organs.

The death of the patient I ascribe to the fact that when the vitality has been lowered to a certain point by malignant tumors, without or with functional disturbances of vital organs, the organism loses its power to sustain more than a certain amount of operating, and death will follow from the yet unexplained exhaustion, in spite of the absence of all the common well-known fatal complications.

DOUBLE CARCINOMA OF THE COLON.

The third and last specimen is from a man between 40 and 50, who had suffered terribly from difficult passages from the bowels for a number of months. Finally a small, almost immovable tumor, appeared to the right of the umbilicus, and later on distention of the small intestines with pain and vomiting. Every half-hour or hour there would be a paroxysm of peristaltic contractions with excruciating pain. He finally asked to be relieved at any risk. On account of his extreme emaciation and weakened condition, I thought it out of the question to attempt extirpation, and resolved to try to relieve him by means of anastosmoses between the intestine above and below the stricture. Laparotomy revealed the tumor to be a carcinoma of the ascending colon; consequently I united the lower part of the distended ileum with the empty transverse colon five to six inches away from the tumor. The patient did not get much relief and died ten days after the operation, growing gradually weaker, as in the other case. The autopsy showed no peritonitis, the omental flap was partially adherent to the intestine, the peritoneum between the plates united, but at the distal end of the plate, in the colon, an island of necrosis of the intestinal wall from pressure-atrophy caused by the plate. Thus in this case perforation of the intestine was only a question of a short time.

The carcinoma of the ascending colon, as the specimen shows, is three inches long and has caused almost complete occlusion of the bowel. The reason why no relief followed the operation was found below the anastosmosis in the splenic flexure of the colon where a second carcinoma had developed, causing as you see, almost complete obstruction of the colon. This second carcinoma was not discovered during the operation, as it was hidden high up, under the spleen. The emptiness of the transverse colon, together with the rarity of a second carcinoma, was the cause of my not suspecting its presence. If it had been discovered, the anastosmosis would have been made between the ileum and the sigmoid flexure of course. The mortality from even palliative operations upon the intestines is large, because, as a rule, the patients do not come to us for operation until they are exhausted by serious intestinal disturbances, usually of long continuance. This is so generally the case, that collapse, even after a short operation, is of frequent occurrence.

Senn's operation of intestinal anastosmosis with the plates does not take any more time than the abdominal operation for artificial anus. The last operation here mentioned was of thirty-eight minutes' duration, from the time of the incision in the abdomen to the dressing of the abdominal wound.

DR. ETHERIDGE: I would like to ask Dr. Fenger if he thinks that, if he had drained the first case, he would have saved her from any gangrenous affection?

DR. FENGER: That is possible.

DR. ETHERIDGE: Why did you not do vaginal hysterectomy.

DR. FENGER: Because the tumor was too large.

DR. ETHERIDGE: If you had a similar case, would you not, after liberating the tumor through the abdomen, make a vaginal hysterectomy?

DR. FENGER: No; not at the same time, because I think that is dangerous—too much operating.

DR. ETHERIDGE: It seems to me that the combination of the two in an operation that, if not prolonged too much, increases the chances of the patient to live. You would have magnificent drainage that way.

DR. FENGER: Is not that a combination of abdominal and vaginal hysterectomy?

DR. ETHERIDGE: Well, cases get well after hysterectomy, even where the abdomen is opened?

DR. FENGER: That is Freund's operation.

DR. ETHERIEGE: Freund's and Schroeder's.

DR. FENGER: What is the difference; is it not the combination of laparotomy and vaginal hysterectomy that brings the mortality up above 60 per cent?

DR. ETHERIDGE: Have the two been done enough to make such a mortality as that; have enough cases been recorded to say that there is a mortality of 60 per cent.

DR. FENGER: What is the difference between that and Freund's operation?

DR. ETHERIDGE: You have better drainage in Freund's operation.

DR. FENGER: From what I think now I would be afraid of that combination.

DR. ETHERIDGE: I have often thought if I should have a case of tumor of the uterus to remove and the adhesions were not enough in the pelvis to fix the cervix immovably, I should make the operation through the abdomen of removing the tumor, put an elastic ligature down as far as possible, then immediately remove the cervix by vaginal hysterectomy depending upon the forceps for control of the hæmorrhage. In that way we could get a magnificant drainage through the vagina.

DR. FENGER: Time would first have to show if such a combination as that would bring Freund's mortality down.

DR. ETHERIDGE: I believe a great many cases of fatal termination of supra-vaginal amputation of the uterus is from lack of drainage, and if after the amputation the balance can be taken out through the vagina, then we can close the abdomen and have the drainage through the vagina. Of course, that could not be done if there were universal adhesions through the pelvis.

DR. FENGER: Frank, of Cologne, who, criticised so much about his ten cases of enucleation, believed that he could peel off the uterus and leave the peritoneum. If that can be done in all cases, which I consider impossible, it can be done much more easily when there are adhesions all around the uterus. In fact, one of Czerny's first operations for uterine sarcoma was done in that way—vaginal enucleation without opening into the peritoneal cavity.

DR. EARLE: Suppose these operations had been successful, how long would they have prolonged the men's lives?

DR. FENGER: The man with double carcinoma would die in due course. In the other case—the colloid carcinoma—it is well known that these carcinomas belong to a comparatively benignant type, and there is nothing against the possibility of a radical cure.

DR. NELSON: I wish to remind Dr. Fenger that he has not explained the cause of gangrene.

DR. FENGER: I will take into consideration first the way in which the wound surface is treated, namely, by a double row of sutures, deep and superficial. That is not enough to cause gangrene. Then comes the liability of these tumors in general to local death, and then comes sepsis. Sepsis plays an important part in the etiology of gangrene, and I do not doubt that sepsis in the collection of blood around the uterus was a main factor in this case.

DR. NELSON: Might there not be an explanation through the nervous system by the destruction of some important nerves?

DR. FENGER: The nervous system is far away. We know so little about gangrene by the destruction of the nerves that we are not able yet to take that up as a factor of much prominence. We hardly know of any gangrene from nervous destruction alone. The decubitus in paralytic patients is a mixture of traumatism and lack of innervation. The same may be said of the destruction of the joints in locomotor ataxy. There is always an element of injury, consequently something more than mere lack of innervation. In this case, I do not believe that the question of nerves would play any part.

DR. NELSON: Could there be a sufficient spasm of the muscular structure left behind to destroy the nutrition of the part?

DR. FENGER: Contractions can, as we know, sometimes make a fibroid die.

THE PRESIDENT: I intended to exhibit, for Dr. Wm. H. Byford, a uterine cysto-myoma possessing all the characteristics of the one just presented by Dr. Fenger, but found to-day that the specimen had been allowed to spoil. It was pedunculated, slightly adherent in places, trabeculated within, and quite full of collections of serum that coagulated upon exposure to the air. The patient was operated upon two weeks ago and is passing through a rapid and easy convalescence. The pedicle was treated extraperitoneally, and the abdominal cavity closed without drainage.

FOREIGN CORRESPONDENCE.

PROF. VON PETTENKOFER.

His views on Cholera and Typhoid Fever—On the Efficacy of Quarantine—On Yellow Fever.

There is no man in Europe to-day that stands as high in Sanitary Science as Prof. v. Pettenkofer. To him Munich owes its diminished mortality from typhoid fever. To him the world owes a vast amount of information concerning epidemics. And to him nations will owe a large immunity from zymotic disease should his views be accepted and put into practice. He is a determined non-believer in the personal contagion doctrine of cholera, typhoid and yellow fever. Hence he sees no practical utility in quarantine. Indeed he believes it to be detrimental in propagating false ideas and exciting needless alarm. His facts tend to explode any credence in the rôle played by drinking water. He sees in the soil and sewage the cause, and the cause only of epidemics. His opinions are backed by over forty years of intelligent study, and by collateral experiments and observations of others in the same line of inquiry. I had occasion to visit the Hygienic Institute this morning in order to obtain from the Professor his views upon the following points:

1. The rôle played by microbes in the production of zymotic disease.
2. The efficiency of quarantine.
3. The best measures to prevent the spread of an epidemic.
4. Views on yellow fever.
5. Vaccination as a preventive.

To his answers, full, concise and convincing, I intend giving the widest circulation possible during the course of the next ten days, because it seems to me that the laity should be amply informed in all matters touching upon the public health. It is not then for this purpose that I now write, but to give briefly some more interesting sparks from a mind that is running over with brilliancy and full of scientific lore. In connection with the etiology of cholera, Prof. v. Pettenkofer took me to his chart-room, to illustrate the fluctuation in the course of typhoid fever in Dantzig and Munich, beginning in the year 1852, and affected by the improvements in the drinking water supply and in improved methods of sewerage. The charts covered rainfall, the amount of ground water, the actual death-rate and the relative death-rate. In the appropriate years were noted the introduction of the new reservoirs, as well as of the improvements in drainage. When the ground-water fell the mortality was larger. When it rose the death-rate was smaller. In 1852, in a population of 100,000, 350 died of typhoid fever. Then the people attacked the water. It was examined by experts and found to be perfectly pure. Würzburg, with a water-supply much less pure, had no typhoid fever. Yielding to public pressure, the city inaugurated a new water supply. The following year the death-rate fell. But the next year and in those following it reached its former formidable proportions. Then Pettenkofer agitated his drainage reforms—the cleaning of the earth, so to speak, and the proper disposal of refuse. Since this time the mortality has steadily diminished, until now the deaths from typhoid fever are extremely rare.

Certain soils tend tend to cleanse themselves rapidly, others require more time, and to utilize this point is one of the achievements of modern Sanitary Science. In Dantzig the conditions were the same as in Munich. A change of drinking water supply gave no better mortality, but when the whole system of drainage was changed for the better the death-rate at once diminished. These epidemics usually follow stagnant water courses, and wherever still, foul water exists zymotic disease will also obtain. Cholera only obtains in districts badly drained—meaning by this every condition of soil for which drainage was inaugurated. In New York, in 1852 (I think it was, the exact date has escaped me), the cholera

attacked only a part of Blackwell's Island and only a part of New York, and that was around the docks and lower part of the city. People visited the infected places, contracted the disease, and died with it in other precincts of the city, and yet no one else in these districts contracted cholera. In Munich during a distressing epidemic certain sections remained entirely healthy.

Dr. Cunningham, of Calcutta, is now passing through the press a large book, in which by a large amount of investigation extending over many years in India, as well as by experiments in the laboratory, he shows Pettenkofer's views to be the only correct ones, and gives the Professor, now covered with years and honors, credit for being the first scientist to place upon a proper footing the question of epidemics. Both of these gentlemen have no belief in quarantines. Their history at any time in the world's history, and whenever enforced, have never shown them to be productive of preventing the spread of an epidemic or of arresting its localized progress. America with a strict quarantine is scourged, while England with no quarantine escapes. The advance of any epidemic can only be arrested by improving the conditions upon which it depends for an existence, and history fails to record a single instance in which it has been stopped in any other way. At a time when Montevideo was suffering disastrously from yellow fever invasion a doctor in charge of the hospital there succeeded in quieting the people who had a superstitious dread of contagion that it *was not* personally contagious by taking his two young children with him each time that he made his visits to the fever wards.

I send this merely as an *avant courrier*. A more elaborate expression of Dr. v. Pettenkofer's views will be published later.

HORATIO R. BIGELOW, M.D.

Munich, Oct. 4, 1888.

DOMESTIC CORRESPONDENCE.

LETTER FROM NEW YORK.

(FROM OUR OWN CORRESPONDENT.)

Hypnotism; its Therapeutic Uses—Therapeutic Value of Hypnotism.

There was an unusually interesting session recently of the Neurological Section of the Academy of Medicine, when the somewhat uncanny subject of hypnotism was dispassionately discussed in a purely scientific and rational manner. The principal paper of the evening was by Dr. C. A. Herter, on *The Therapeutic Uses of Hypnotism*. He said the employment of this agency was attended with considerable difficulty on account of the prejudice existing against it in the minds of the profession, as well as the public; and it was perhaps impossible at the present time to arrive at any positive conclusions as to its proper place in medicine. Hypnosis resembled ordinary sleep, with the element of "suggestion" added. Suggestion was effective, however, in the waking state also, as shown in the so-called Christian Science and faith cure, as well as to some extent as regards homœopathy.

Dr. Herter adopted the classification of Lieboldt, who makes six divisions of hypnotic trance, according to the degree of its intensity, in contradistinction to that of Charcot into catelepsy, lethargy and somnambulism, and said that he did not think that Charcot's cases should be taken as a basis for conclusions because they were, as a rule, in trained subjects. He then described the proper method of inducing hypnosis, as advocated by Lieboldt and Bernheim. The operator should first secure the confidence of his patient, and then tell him to look him steadily in the eye and think of nothing but going to sleep. The process could be materially aided by suggestive remarks addressed to the patient and by placing two fingers upon the face; the fingers being finally pressed gently upon the eye-lids. In order to obtain good results it was necessary to watch the patient very closely, and to suit the words to the symptoms of sleep as they developed themselves. The *rationale* was very simple. Verbal suggestion in the waking state made the subject believe that he was falling asleep, and the method closely resembled the natural process.

Of the various conditions in which hypnotism has been employed therapeutically Dr. Herter first spoke of hysteria. More attention, he said had been given to its use in this connection than any other, but notwithstanding all that had been written upon it, the special indications for resorting to it are still far from clear. It was probable that hypnotic suggestion might be of more or less service in all forms of hysteria; but, as a rule, recovery, if secured, was not permanent. Its effect on hysterical amaurosis and amblyopia was extremely interesting, and in many cases a very marked improvement in vision had attended its persevering use. Of the motor paralysis none are more benefited by it than abductor paralysis of the larynx. In hysterical convulsions the number and novelty of the attacks were greatly diminished. No one, he thought, need ever be discouraged by failure to secure hypnosis at the first trial in any case of hysteria. The prospect of a cure would depend on various circumstances, but at all events hypnotism would in many instances serve as a useful adjunct to other treatment.

Not much was to be hoped from it in hystero-epilepsy, and the same was true as regards ordinary epilepsy. In some cases of chorea he stated that the effect on the movements was rapid and marked, and with perseverance a cure might be

effected. A number of daily sittings was generally required, and in some cases it was necessary to keep them up for months. In any case of supposed cure, however, there would always arise the question whether the disease had not simply run its course and ended spontaneously. There was the most ground for hope in those cases in which the movements were general, and the previous existence of rheumatism and endocarditis did not seem to render the chances of relief less favorable. In insanity the results from hypnotism had not for the most part been satisfactory. He had seen it tried quite extensively at Zurich, but had never observed any benefit from it that was not evanescent. In delirium tremens its effect was often excellent, and quiet useful sleep was sometimes secured by it in cases in which the same result could only have been attained by the use of drugs in doses so large as to be dangerous. Alcoholism constitutes one of the most hopeful fields for its employment. The *séances* should be given twice daily for several months, and in many cases was said to become inspired with a complete disgust for drink. The opium, chloral, cocaine and tobacco habits could probably be also successfully treated in the same way; but in all these cases surveillance in an asylum was, of course, an indispenable feature of the treatment.

Masturbation and other bad habits, he continued, had been cured by hypnotism, and perhaps no more satisfactory results had ever been observed from its use than in incontinence of urine in children. Lieboldt had employed it in 77 cases, with a very large percentage of cures. The average age of the patients was 7 years, and in two of those cured there was incontinence of fæces in addition to the enuresis. The results obtained were the more gratifying from the fact that many of the cases had resisted all the ordinary methods of treatment. In various rheumatic affections Bernheim and others had reported partly satisfactory results. In some chronic joint cases it was claimed that not only was the pain diminished, but that the nutrition of the joint was actually improved by the treatment. Dr. Herter thought it very doubtful, however, whether any real effect upon the joint was produced. In neuralgia much benefit could be expected in recent cases, but if the trouble was chronic there was less chance of success. Still, in some old cases of sciatica good results had been claimed, and at all events the method might often be of service in securing sleep, even if no permanent benefit could be looked for. In functional headaches it was of decided advantage, and in certain instances of migraine he had been successful in mitigating and cutting short the attacks by its aid.

At first sight it might seem that such an agency as this could have no effect upon a function like menstruation; but he said it had been practically demonstrated that in some cases not only the interval between the periods, but the length of the periods themselves, could be absolutely controlled by it. The number of individuals in which such results could be looked for, however, was comparatively few. As to the employment of hypnotism in surgery as a substitute for the ordinary anæsthetics, the number of patients, with the exception perhaps of children, in whom it was available for major operations was no doubt small; especially as the anticipation of having an operation performed was extremely apt to interfere with the success of the hypnosis. On the whole, he thought its employment in this field was not to be recommended, except in cases where, for any reason, the use of anæsthetics was contraindicated. He had, however, seen a molar removed under its influence without the patient's suffering any pain or, indeed, having any remembrance afterward of the extraction of the tooth. In parturition Dr. Herter said that sufficient experience had now been accumulated to prove that hypnotism is in no wise prejudicial to efficient uterine contraction, and that it has no tendency to produce *post-partum* hæmorrhage or other bad results. At the same time he thought it was in no way comparable to chloroform in labor, and ought only to be used in those rare cases where the usual anæsthetics are contraindicated. In insomnia it could often be employed with good results, but it sometimes required months of persevering treatment to effect a cure. In cases where drugs had been resorted to it could be used in addition to the habitual remedy; the dose of the latter being gradually diminished, and perhaps finally replaced by a placebo.

Among the objections that had been raised against the employment of hypnotism he mentioned the following: 1. It tends, in certain susceptible subjects, to produce temporary mental alienation; some individuals thus developing hallucinations during wakefulness. 2. Some individuals fall asleep spontaneously; showing a tendency to auto-hypnotism. 3. Some can be readily put to sleep by any one who chooses to do so. Such bad results as these Dr. Herter thought could, for the most part, be entirely antagonized by suggestion.

In conclusion, he proposed the following general rules for those desiring to practice suggestive therapeutics:

1. Never hypnotize any subject without first obtaining his or her formal consent.

2. Always hypnotize in the presence of a third person.

3. Never give any suggestions other than those which are necessary for the patient's improvement in health.

Public exhibitions of hypnotism he believed should be prohibited by law, and the use of this agency confined exclusively to the medical profession. One advantage connected with its employment in medicine was that it does not lose its

effect by repetition. On the contrary, the subject becomes more and more readily influenced by it the oftener he tries it. From what had been said he thought it could be seen that suggestive therapeutics was not a delusion, and he said he had no doubt that before many years it would be assigned a regular position in medical service. At the conclusion of his paper Dr. Herter demonstrated on a female subject the method of inducing hypnosis.

The Chairman of the Section, Dr. W. R Birdsall, next read the report of a case of violent tremor of the right forearm, which he believed to be of hysterical origin, although the patient was a young man, and which was successfully treated by hypnotic suggestion. In concluding it he said he thought we were not justified in wholly disregarding a method of treatment which had already shown itself useful in many nervous troubles; though, of course, he did not mean to contend that we should by any means attempt to treat all classes of cases by hypnotism, and thus place ourselves on a level with the faith healers and Christian science people.

Dr. Osgood Mason then read a paper based on six cases of hypnotic treatment, in which he gave his deductions as follows:

1. It is of decided value as a therapeutic agent.
2. Sleep is not always necessary for the production of the greatest benefit.
3. It has the effect of equalizing the circulation.
4. It also regulates and equalizes nervous action.

Corollary.—The equalization of the circulation is the result of the equalization of nervous action.

The last paper was one on *The Therapeutic Value of Hypnotism*, by Mr. Corey, of Boston, Chairman of the Committee on Hypnotism of the American Society for Psychical Research. In the course of it he spoke particularly of the results obtained by Mr. Voisin in certain cases of insanity. He also referred to many of the affections spoken of in Dr. Herter's paper, and expressed the opinion that hypnotism was destined to prove of great service in properly selected cases in medical practice. In the latter part of it he dwelt at some length upon the medico-legal aspect of the subject, and spoke of the danger that susceptible individuals might be caused by the unscrupulous to commit criminal acts while in a state of hypnotic trance.

In the discussion that followed Dr. Kremer, after relating some cases that he had had, stated that in one instance he had considerable difficulty in arousing the patient, while in several others he found that the treatment gave rise to headache and malaise. On the whole, he said, his experience with the method, which was confined to six or eight cases of a neurasthenic or hysterical character, had not been very favorable.

Dr. C. L. Dana referred to two or three cases, and then said that it was rather a curious coincidence that just at the time when the convention of the "Christian Alliance," composed of those who believed in faith healing, was treating the subject of suggestive therapeutics empirically, it should be discussed at the Academy of Medicine from a rational point of view. He said that five or six years ago, when the method of Bray was in vogue, he had paid considerable attention to hypnotism, and he had known of one subject, a young girl of nervous temperament, who was affected with convulsions in consequence of the treatment. While the method of Bernheim and Lieboldt was much less objectionable, he did not think the practical application of hypnotism was of very wide range. There were comparatively few individuals who were suitable subjects for treatment by it, and there were not many physicians who were willing to devote the time necessarily required by it. Dr. Dana thought the so-called faith cure and Christian science should be prohibited by law, and expressed his opinion that it was better for a person to remain ill rather than to be cured by such agencies, because they tended to produce paresis of the will and eventually bring about a certain amount of volitional degradation.

Dr. Herter said, in reply to Dr. Kremer, that he had never found any difficulty in arousing a patient from the hypnotic sleep, although he had seen the method tried in over 100 subjects. The only exception was in an individual who had been in a spontaneous condition of trance previously. He did not think this a valid objection to hypnotism, and as to the headache and malaise referred to, he believed these could always be prevented by suggestion before allowing the patient to waken. He agreed with Dr. Dana as to the bad effects of Bray's method with the bright light, which had been known to give rise to epilepsy and convulsions; but said he was not aware of any evil results having been noted from the method now employed. He had never noticed any depressing tendency about the treatment, and, as far as his experience went, he believed it was comparatively easy to hypnotize quite a large proportion of individuals in the community.

P. B. P.

MISCELLANEOUS.

DIPHTHERIA SPREAD BY CATS.—Domestic animals have often not only been suspected but found guilty of spreading infection. In his report on the recent sustained prevalence of diphtheria in Enfield, Dr. Bruce Low of the medical department of the local government board incidentally states that during the continuance of the epidemic cats were observed to suffer in considerable numbers from illness, and in December, 1887, and in January, 1888, there was a large mortality among those animals, so much so that the attention of the dust contractor was directed to it. He stated that never in his previous experience had he seen so many dead cats in the

dust heaps. Some households seeing their cats ill, destroyed them. Though there were no known cases of diphtheria occurring in the practice of the veterinary surgeons at Enfield, yet they saw many cases of "influenza" at this time among animals. The following is an illustration of the possible connection between diphtheria in children and in cats: A little boy was taken ill with what turned out ultimately to be fatal diphtheria. On the first day of his illness, the cat, which was in the room at the time, licked the vomit on the floor. In a few days (the child meanwhile having died) the animal was noticed to be ill, and her sufferings being so severe and so similar to those of the dead boy the owner destroyed her. During the early part of its illness this cat had been let out nights in the back yard, as usual. A few days later the cat of a neighbor, who lived a few doors further off was noticed to be ill. It had also been out in the back yards at night. The second animal, which, however, recovered, was the pet and playfellow of four little girls, who, grieved at the illness of their favorite, nursed it with great care. All four girls developed diphtheria, the mother being convinced that they got it from the cat; and, indeed, no other known source of contact with infection could be discovered. It is easy to imagine cats catching infectious diseases like diphtheria when we remember how often milk and other unused food from the sick-room is given to the cat, or by some people thrown out in the back-yard for the benefit of their neighbors' cats if they have none of their own. It is a frequent occurrence to see children carrying cats in their arms, and even kissing them. It is obvious that if the cats were ill with diphtheria the children, under such circumstances, would almost inevitably contract the disease.—*London Sanitary Record.*

ACTIVITY OF THE SCARLET-FEVER POISON AFTER A YEAR.—Dr. J. Brook, Surgeon U. S. Army, of Fort Monroe, Va., communicates the following case: "A girl aged about eight, living at this place, was some months ago attacked by scarlet fever, the disease running a typical course. For a long time no possible source of contagion could be discovered. The child had not been absent from home, had been with no one lately exposed, and no other case was known to exist anywhere in the vicinity. Subsequently I learned that one of the house-servants had nursed a case of scarlet fever in a distant city just about a year before. After the case terminated she packed some of her things, including some clothing then worn, in a trunk, and left the place. A year later she had the trunk sent to her here, opened it, and took out the contents, the little girl being present and handling the things. Very soon after the latter was attacked, as stated.

"As fixing the period of incubation, it would be interesting to know precisely how many days passed from the time the trunk was opened until the disease appeared; but I was unable to determine that period satisfactorily."—*The Medical Record.*

INQUESTS OF DEATHS FROM PREVENTABLE DISEASES.—At the Paddington Vestry on Tuesday last a resolution was submitted from the Sanitary Committee, setting forth that several cases of death from preventable diseases having been reported to them under circumstances which suggest that the cause has been defective sanitary arrangements, and that the Committee is of opinion that in all such cases application should be made to the coroner to hold an inquest as to the cause of death, and recommending that a copy of the resolution be sent to all the vestries and district boards of the metropolis. It was further suggested that copies should be forwarded to the medical practitioners in their respective districts. Dr. Danford Thomas approved of holding an inquest where there was evidence of gross sanitary neglect.—*British Medical Journal*, Sept. 22, 1888.

DR. E. MILLER REID has recently heen elected to the Chair of Physiology and Hygiene in the Baltimore University School of Medicine.

DR. F. R. CAMPBELL, a talented physician and writer of Buffalo, died of typhoid fever on September 14, at the early age of 28 years.

DR. HARVEY JEWETT, of Canandaigua, N. Y., died on September 4, aged 79 years.

Official List of Changes in the Stations and Duties of Officers Serving in the Medical Department U. S. Army, from October 13, 1888, to October 19, 1888.

Capt. Edward T. Comegys, Asst. Surgeon, is relieved from duty at Madison Bks., New York, and will report in person to commanding officer, Ft. Bayard, N. M., for duty at that post, relieving First Lieut. William D. Dietz, Asst. Surgeon.

First Lieut. Dietz, on being relieved by Capt. Comegys, will report in person to commanding officer, Alcatraz Island, Cal., for duty at that post, reporting by letter to the commanding General Dept. of California. Par. 18, S. O. 240, A. G. O., Washington, October 15, 1888.

By direction of the Secretary of War, the following named officers of the Medical Department will report in person, on October 23, 1888, to the President of the Army Medical Examining Board, Army Building, New York City, for examination for promotion: Capt. John de B. W. Gardiner, Asst. Surgeon; Capt. William C. Gorgas, Asst. Surgeon; Capt. C. N. Berkeley Macauley, Asst. Surgeon; First Lieut. W. L. Kneedler, Asst. Surgeon; First Lieut. Edgar A. Mearns, Asst. Surgeon. Upon completion of their examination the officers named will rejoin their proper stations. Par. 1, S. O. 239, A. G. O., Washington, D. C., October 13, 1888.

Capt. Benjamin Munday, Asst. Surgeon, is relieved from duty at Jefferson Bks., Mo., and will report in person to the commanding officer, Ft. Sisseton, Dak., for duty at that post, relieving First Lieut. John L. Phillips, Asst. Surgeon, and reporting by letter to the commanding General, Dept. of Dakota. Par. 11, S. O. 242, A. G. O., Washington, October 17, 1888.

First Lieut. Phillips, on being relieved by Capt. Munday, will report in person to the commanding officer, Ft. Lyon, Col., for duty at that post, reporting by letter to the commanding General, Dept. of the Missouri. Par. 11, S. O, 242, A. G. O., Washington, October 17, 1888.

Official List of Changes in the Medical Corps of the U. S. Navy for the Week Ending October 20, 1888.

Surgeon A. F. Magruder, ordered to Marine Bks., Washington, D. C.

Asst. Surgeon E. P. Stone, detached from the "Richmond" and to the "Minnesota."

Asst. Surgeon J. F. Keeney, detached from the "Minnesota" and to the "Richmond."

Asst. Surgeon A. M. D. McCormick, detached from the Bureau Medicine and Surgery, and to the "Vermont."

Surgeon A. M. Moore, detached from naval station, New London, Conn., and to the "Kearsarge."

P. A. Surgeon A. A. Austin, ordered to naval station, New London, Conn.

Official List of Changes of Stations and Duties of Medical Officers of the U. S. Marine Hospital Service, for the Two Weeks Ending October 20, 1888.

P. A. Surgeon S. T. Armstrong, granted leave of absence for sixteen days. October 17, 1888.

Asst. Surgeon R. M. Woodward, when relieved at Marine Hospital, Boston, Mass., to proceed to Marine Hospital, Chicago, Ill., for duty. October 12, 1888. Granted leave of absence for thirty days. October 17, 1888.

Asst. Surgeon A. W. Condict, relieved from duty at Marine Hospital, Chicago, Ill.; ordered to Marine Hospital, Boston, Mass. October 12, 1888.